CURRENT SCIENCE

Vol. XV] AUGUST 1946 [No. 8

THE DIAMOND AND ITS TEACHINGS

THE Indian Academy of Sciences has once again made scientific history by the publication as its *Proceedings* for July 1946 of a symposium of original papers devoted exclusively to the physics of the diamond. Twenty-one memoirs by eight authors make up a volume of 197 pages illustrated by 23 full-page plates and numerous figures in the text. A similar symposium was issued in May 1944 which contained seventeen papers by eleven authors and ran to a total of 153 pages and 30 full-page plates. This was reviewed in *Current Science* for June 1944. Eight separate papers on diamond by various authors also appeared in the *Proceedings* during the latter half of 1944 and the first half of 1945. The enthusiasm manifested in this output of research possibly needs a few words of explanation. There is good reason to believe that the investigation of the structure and properties of diamond would lead to a deeper understanding of the basic principles of organic chemistry and of crystal chemistry. The crystal forms and the genesis of the diamond also offer problems of great interest to the mineralogist. And to the physicist interested in the theory of the solid state, diamond presents an almost illimitable field for fruitful research. For it is at once the most representative and the most exceptional of solids—representative because of its elementary nature and the simplicity of its structure, and exceptional because in spite of these qualities, it exhibits many remarkable properties and a fascinating variety of behaviour.

The principal difficulty in such studies is that of obtaining suitable material. Diamond shows wide variations in some of its most characteristic properties. Hence, the experimenter should have at his disposal a fairly large collection of specimens. This fact and the expensiveness of the best material is a discouragement to the investigator. The work pursued during the past few years at Bangalore has been made possible by the material gradually got together by the present writer. The additions made recently to the collection have been particularly useful and have enabled definite conclusions to be reached on many important questions.

The Crystal Forms of Diamond

Diamond presents some peculiar puzzles to the crystallographer. One of these is the strongly marked curvature of the faces of the crystals which is a very general feature, while in some specimens both plane and curved faces appear in combination. It is obvious that a crystal exhibiting curvature in some or all of its faces cannot appropriately be described in the usual terminology of geometric crystallography and that a new approach is therefore needed. In the introductory paper of the symposium, it is shown that the proper basis for description and classification of the forms is the pattern of sharply-defined edges seen dividing the curved surface of the diamond into distinct sections. These edges lie in the planes which contain the valence directions within the crystal taken two a time. There are six such planes and if they are drawn passing through a given point in space, their intersections with a closed surface surrounding it would divide up the surface into 24 triangular areas. In the ideal pattern thus derived, there are six points on the surface at each of which four edges meet and eight points where six edges meet. In a general way, these are the features actually seen on the surface of the diamonds, though there are certain modifications in detail. The configuration of the edges is found to be related in a remarkable way to the form of the diamond and the valence directions. The vertices or prominences of a crystal form are invariably points where four or six edges meet sharply. *Per contra*, on relatively flat areas of the surface, the edges are always inconspicuous and show a tendency to meander in their courses and to intersect in a somewhat haphazard fashion forming broken zig-zags. The form of the crystal approximates to a regular rhombic dodecahedron when all the prominent edges are nearly straight and parallel to the valence directions. Even in such cases, however, the rhombic faces are traversed by inconspicuous edges dividing them in two, thereby securing the usual subdivision of the surface into twenty-four distinct sections. This is an example of the general principle that the edges are most conspicuous when they nearly coincide with a valence direction and least conspicuous when they deviate largely from it.

The Crystal Symmetry of Diamond

The six planes containing the valence bonds are also the symmetry planes of the tetrahedral carbon atom. Octahedral symmetry for the crystal would demand three additional planes of symmetry, *viz.*, the axial planes of the cubic structure. If all these nine planes are drawn through a point in space, they would divide up an enclosing surface into forty-eight sections and not twenty-four. There is no hint or suggestion in the Panna diamonds of any edges lying in the axial planes of symmetry, and it may, therefore, reasonably be inferred that the crystal symmetry of those diamonds is that of the tetrahedral and not that of the octahedral class. The correctness of this inference is confirmed by the fact that the characteristic features of hemihedry are very clearly exhibited by numerous specimens. In particular, the configuration of the edges at the two ends of each triad axis of symmetry are found to be notably different, one end appearing as a sharp vertex or prominence of the crystal, while at the other end the surface is a flattened dome. Four different views of such a "tetrahedroid" diamond are shown in Fig. 1.

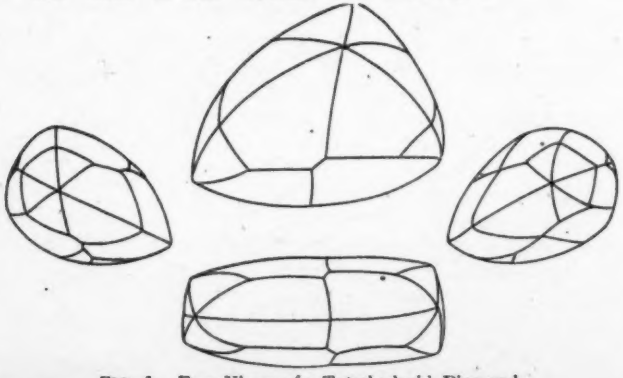


FIG. 1. Four Views of a Tetrahedroid Diamond.

It may seem surprising that diamond which consists of atoms all of the same kind has the same crystal symmetry as zinc blende which, as is well known, is a polar crystal exhibiting piezo-electric properties. This point is discussed in a paper by Mr. G. N. Ramachandran and it has shown that diamond may possess tetrahedral symmetry without being a polar crystal; a suitable distribution of the electron atmospheres would secure this result without involving any differences in the total charge attached to the individual atoms. It must not, however, be supposed that diamond is *invariably* a crystal of the tetrahedral class. The evidence of the crystal forms shows only that the majority of diamonds possess the lower symmetry and that diamonds which unambiguously exhibit the higher or octahedral symmetry are much less common. There is also distinct evidence from the crystal forms that the interpenetration of the positive and negative tetrahedral structures is a very frequent occurrence.

Atomic Vibration Spectrum

Being the typical valence crystal, diamond is particularly well-suited to be a test-case for theories of solid behaviour. In particular, its optical properties in the ultra-violet, visible and infra-red regions of the spectrum are of extra-ordinary interest in relation to such theories. It is not surprising, therefore, that these properties have come in for a good deal of attention. Indeed, the majority of the papers in the symposium are concerned with such questions. In particular, experimental evidence is presented in papers by Dr. R. S. Krishnan and Mr. K. G. Ramanathan which very definitely clears up the fundamental problem of the nature of the atomic vibration spectrum in a crystal lattice.

In principle, the problem of the vibration spectrum of a crystal lattice is a simple one. For, the structure of a crystal is three-dimensionally periodic in space and comes into coincidence with itself when given unit translations in turn along each of the three axes of the space-lattice. The characteristic modes of vibration of the atoms should accordingly also exhibit the same property. Since the phases of vibration of the atoms in a normal mode are either the same or opposite, the result of a unit translation would be that the phases of the atoms brought into coincidence either all remain the same or else are all reversed. We have thus $2 \times 2 \times 2$ or 8 distinct sets of cases to be considered. The set in which the phase of the vibration is the same in all the units of structure includes $(3p - 3)$ modes of vibration (excluding simple translations), while the remaining sets give us $21p$ modes, p being the number of non-equivalent atoms per unit cell. Thus the result emerges that the crystal structure has $(24p - 3)$ modes of normal vibration, each having a definite monochromatic frequency. These modes must of course be regarded as very highly degenerate, thereby taking account of the immense number of atoms whose vibrations they describe. The three modes left out in this enumeration represent the degrees of freedom carried over into the elastic or low-frequency spectrum of the vibrations of the solid regarded as a continuum.

The nature of the vibration-spectrum, as revealed by all the spectroscopic investigations on the scattering of light in crystals so far made, is seen to be in perfect agreement with the foregoing indications of the theory, provided the disturbing effect of the thermal agitation on the postulated regular ordering of the atoms is taken into account. The finite amplitudes of vibration and the resulting anharmonicity have also to be considered, as they give rise to the possibility of overtones and combinations of the $(24p - 3)$ modes. What is needed for a complete demonstration of the theory is observational evidence that besides the $(3p - 3)$ modes, the remaining $21p$ modes with sharply defined frequencies and their overtones and combinations also exist. Clear evidence on this point is furnished by the series of investigations recently carried out by Dr. R. S. Krishnan and published in the *Proceedings* of the Academy under the serial title of "Raman Spectra of the Second Order in Crystals". The results obtained by him with diamond are particularly significant and conclusive and will now be referred to.

Diamond contains two non-equivalent atoms in its unit cell, and hence the $(3p - 3)$ modes give us three frequencies which reduce to one by reason of the cubic symmetry of the crystal. This is clearly the mode with the frequency of 1332 cm.$^{-1}$ revealed by the earlier studies. [One of the papers in the symposium gives accurate measurements of the frequency of this mode over a wide range of temperature and reveals both a diminution of

this frequency as also a steadily increasing width of the line with rising temperature. These data have also been discussed in relation to the thermal expansion of diamond for which a set of accurate data is presented.] The remaining eight modes are found to be inactive in light-scattering in agreement with theory, which, however, indicates that they may appear as overtones and combinations in the second order spectrum and this again is found to be the case (Fig. 2). The figure is a microphotogram and shows a series of well-defined peaks in the positions expected, a relatively feeble continuum overlying them, which evidently owes its origin to the numerous unresolved combinations of the discrete frequencies both amongst themselves and with the lower or continuous part of the vibration-spectrum of the lattice. Particularly noteworthy and significant is the fact that the peaks observed by Dr. R. S. Krishnan in the second-order spectrum of light-scattering agree closely in position with the series of sharply defined peaks observed earlier by Robertson, Fox and Martin in the infra-red absorption spectrum of diamond when examined under adequate resolving powers.

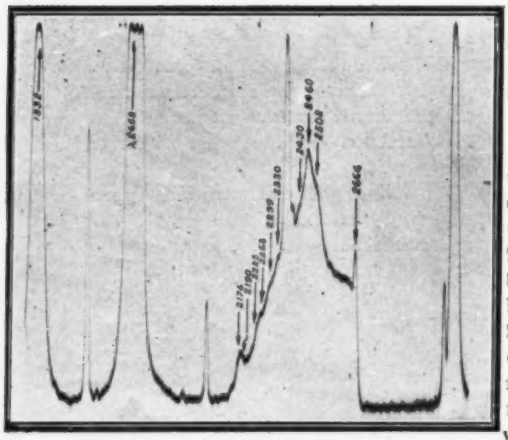


FIG. 2. Second Order Raman Spectrum of Diamond.

INFRA-RED ABSORPTION SPECTRUM

One of the most firmly established results of physics is the relationship between the various physical properties of a crystalline solid and the symmetry of its structure of which the external form of the crystal is an indication. The tetrahedral symmetry of diamond involves as a necessary consequence that the fundamental vibration-frequency of the lattice (1332 cm.$^{-1}$) should be active in infra-red absorption, while *per contra*, if diamond had an octahedral symmetry of structure, the same vibration would be inactive in such absorption. It had long been known that some diamonds exhibited an absorption-band in the 8 μ

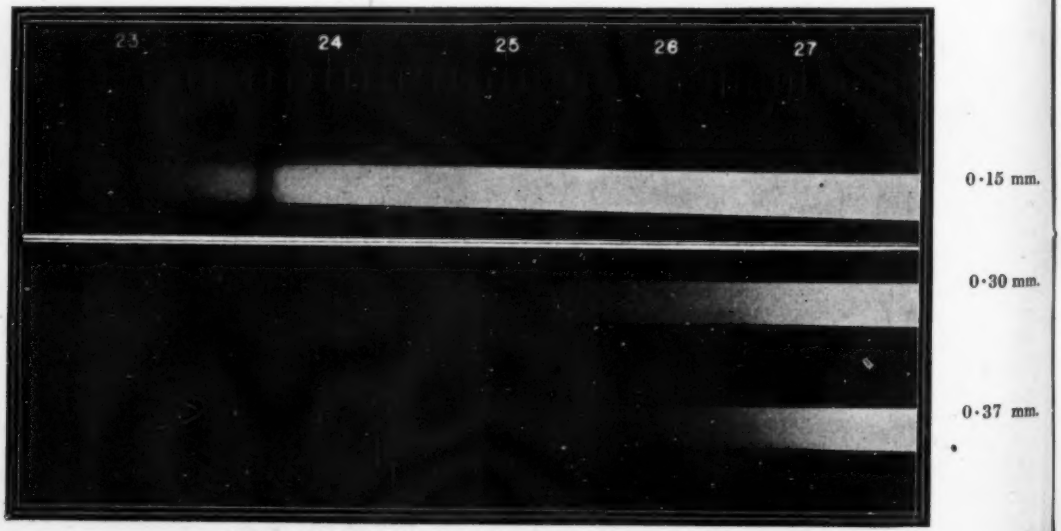


FIG. 3. Ultra-Violet Transparency of Thin Diamonds.

region, while others apparently did not, but evidently owing to the use of unsatisfactory material, the differences in behaviour hitherto recorded were not as clear as was to be expected, and the data also left much to be desired in other respects. In two papers appearing in the symposium, Mr. K. G. Ramanathan has cleared up the position fairly completely, both by obtaining and presenting new experimental data with numerous specimens of well-ascertained structure, and also by a detailed discussion of his results. As *Current Science* for July 1946 contained a report of his work, it is unnecessary to describe it in detail here. It will suffice to remark that his work settles a very important issue, *viz.*, the existence of allotropic modifications of diamond having tetrahedral and octahedral symmetry respectively and the activity of the frequency 1332 cm.$^{-1}$ in the former and its inactivity in the latter. Of particular importance also is the explanation put forward by him of the observed structure of the infra-red absorption bands in the 8 μ and 5 μ regions in the light of the theory of the vibrations of a crystal lattice discussed earlier. The demonstration of local variations in infra-red transparency over the area of cleavage plates of diamond is another noteworthy contribution made by this author.

The Electronic Spectrum of Diamond

That variations exist in the transparency of diamond in the ultra-violet region of the spectrum has long been known. A new complexion is given to the subject by the result established by Mr. K. G. Ramanathan that even the diamonds which in moderate thickness are opaque to radiations below λ 3000, are transparent upto λ 2250 provided their thickness is sufficiently reduced (Fig. 3).

Equally remarkable is the result established by the same author that diamonds of the same type if employed in sufficient thickness completely cut off all wave-lengths below about λ 4140 and also exhibit a whole series of discrete absorption lines and bands between this wave-length and λ 4800. New detail has also been recorded by him in the absorption-spectra of such diamonds right up to the limit of transmission, *viz.*, λ 2240, as observed at liquid air temperature.

From these studies, taken in conjunction with the results of the earlier investigations by Dr. P. G. N. Nayar, by Mrs. K. Sunanda Bai

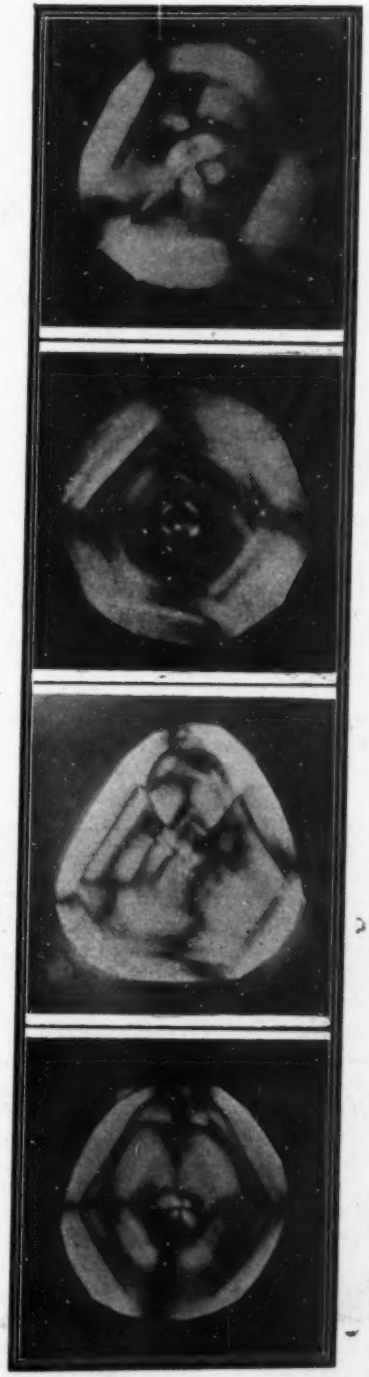


Fig. 4. Birefringence patterns in Diamond.

and Miss Anna Mani, published in the *Proceedings* of the Academy, a very remarkable fact emerges, *viz.*, that diamond exhibits a whole series of sharply defined electronic frequencies in absorption between λ 2240 and λ 5359. Numerous sharply defined emission frequencies have also been recorded by Miss Mani in the luminescence spectra of diamond in the range between λ 4060 and λ6358, and in the majority of cases, corresponding absorption frequencies have been observed. The intensity with which these absorption and emission frequencies are recorded with different diamonds differ enormously. But there can be no doubt that they are all characteristic of diamond itself and that the variations arise from the same causes which give rise to the variations in infra-red absorption strength.

Allotropic Modifications of Diamond

It is a remarkable fact that though diamond is a cubic crystal it often exhibits birefringence, and that this not frequently takes the form of geometric patterns very clearly related to the crystal structure. Fig. 4 is an illustration of four large flat cleavage plates of diamond as viewed between crossed polaroids exhibiting this geometric character in a very striking fashion. These plates are all of the type of diamond which in moderate thicknesses is opaque to wave-length less than λ 3000. Diamond which is completely transparent up to λ 2250 invariably exhibits a characteristic and wholly different type of birefringence that is finely streaky in character. It must not be thought, however, that diamond is always birefringent. Indeed, this is not the case, and in the writer's collection there are several fine specimens of non-birefringent diamond, and these have been very successfully used by Mr. S. Ramaseshan for the studies of the Faraday effect in diamond described by him in the symposium. They belong to the tetrahedral or infra-red opaque type of diamond.

In the earlier symposium, the present writer suggested that birefringence in diamond—except when due to obvious cracks or other defects—arises from the juxtaposition in the same specimen of different allotropic modifications of diamond. This suggestion has been confirmed and placed on a quantitative basis by Mr. G. N. Ramachandran using a very ingenious method. The cleavage plate of diamond under examination is placed on the Federov stage of a petrographic microscope, using where necessary the auxiliary glass spheres. Very remarkable changes in the nature of the birefringence pattern are observed when the plate is tilted on the stage and also when the stage is rotated. A Babinet compensator inserted in the microscope so that the image of the diamond is focussed in its plane enables the sign and magnitude of the birefringence to be evaluated under these conditions.

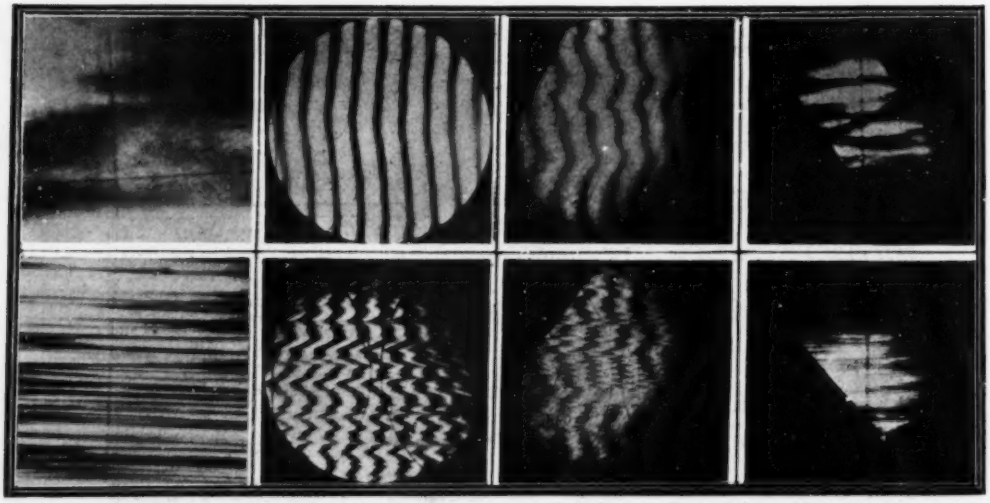


FIG. 5. Birefringence in Diamond observed with Federov Stage and Babinet Compensator (Octahedral Diamond)

In this way it has been shown that birefringence *invariably* arises from the presence of layers lying in the octahedral or dodecahedral planes and different from the material on either side. The upper and lower pictures in Fig. 5 represent the effects observed in this way of tilting the Federov stage with or without the Babinet compensator in the field. The diamond in this case was of the octahedral variety.

Fig. 6 shows similar pictures of a plate of tetrahedral diamond containing intruding octahedral layers. (The nature of the intrusion was verified in this case by the observation of the ultraviolet transparency of the layers). The picture shows that some of the layers have a greater and some a lesser refractive index than the rest of the diamond. These pictures clearly prove the existence of two forms of octahedral diamond, as had been suggested earlier by the present writer.

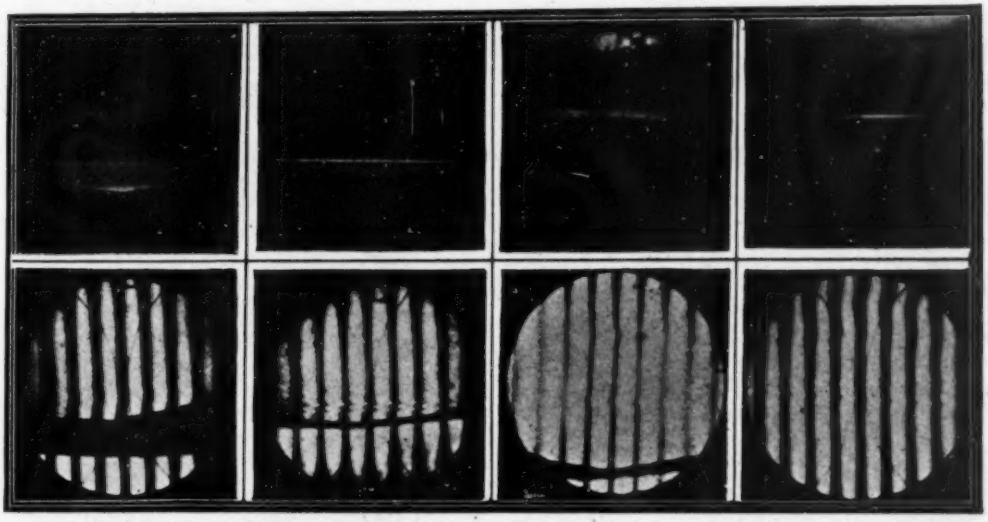


FIG. 6. Birefringence observed with Federov Stage and Babinet Compensator, showing intruding Octahedral layers in Tetrahedral Diamond.

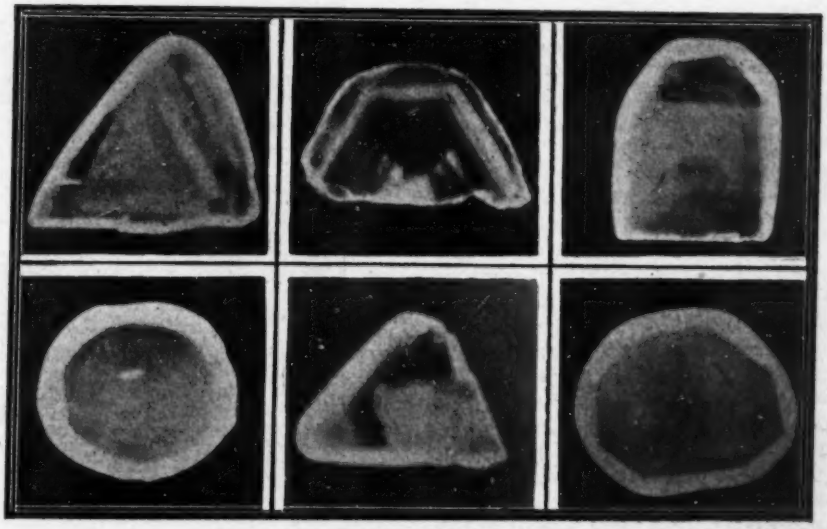


FIG. 7. X-ray Luminescence Patterns.

The Luminescence of Diamond

The fluorescence of diamond excited in various ways and the subsequent after-glow or phosphorescence form the subject of no less than six papers in the symposium which deal with these subjects from different points of view. Mr. G. N. Ramachandran has made a detailed study of the luminescence as excited by X-radiation, the mechanism of its production with special reference to the strength and quality of the X-radiation, and also the remarkable differences between such luminescence and that excited by ultra-violet light in respect of the spectral character of the emission and other features. With Mr. G. R. Rendall's paper are reproduced the luminescence patterns of no fewer than 19 cleavage plates, photographed separately to exhibit the "blue" and the "yellow" luminscence patterns. These are set alongside the ultra-violet transparency patterns and the birefringence patterns of the same diamonds, so as to exhibit the notable resemblances and differences between these patterns. Fig. 8 is an example of such patterns, but the difference between the "blue" and "yellow" patterns and the analogy between the latter and the birefringence patterns are usually much more striking.

Mr. V. Chandrasekharan in one of his papers records a series of phosphorescence patterns obtained by the method of contact photography and shows that only the "blue" patterns as seen in fluorescence are recorded in phosphorescence, though the colour of the latter is "yellow" and not "blue". Mr. Chandrasekharan also describes a series of interesting studies on the activation of diamonds by short-wave ultra-violet rays and the release of such activation energy in the form of "'blue" luminescence by the impact of red light or other long-wave radiation or by the action of heat. These studies as well as the other papers noticed above demonstrate beyond all possibility of doubt that luminescence is a characteristic of diamond itself and not due to any extraneous impurities. Messrs. Ramachandran and Chandrasekharan have a joint paper, the results of which seem to indicate that the luminescence of diamond owes its origin to "forbidden" electronic transitions between various sharply defined energy levels characteristic of its crystal structure. That the intensity of luminescence varies enormously from specimen to specimen is not inconsistent with this view. This is indicated by the fact that increased

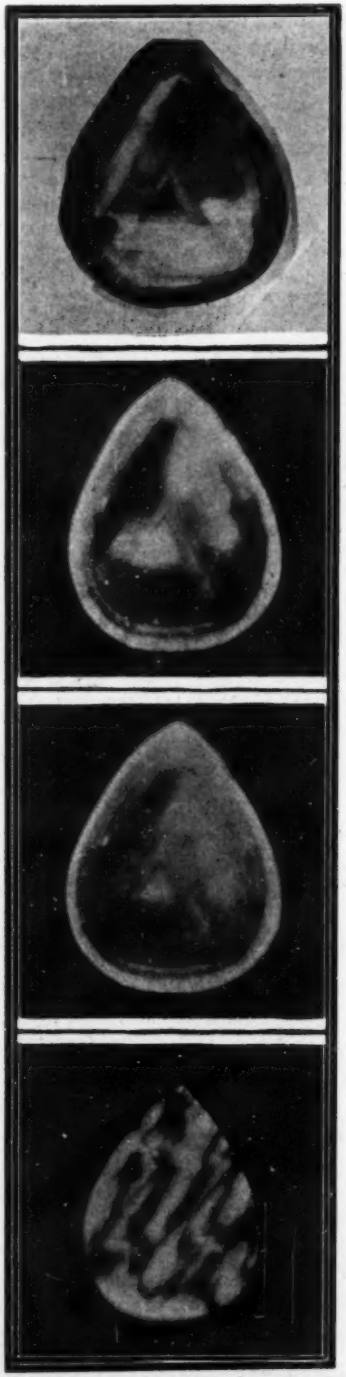


FIG. 8. Ultra-violet transparency, blue luminescence, yellow luminescence and birefringence patterns of a cleavage plate of diamond.

intensity of blue luminescence goes hand in hand with an increased mosaicity of crystal structure, as is shown in Mr. Ramachandran's paper on X-ray topographs. Local variations in mosaicity appear also to be responsible in some cases for the local variations in photo-conductivity discovered by Mr. Achyutan in the cleavage plates of diamond.

THE GENESIS OF THE DIAMOND

Reference should also be made to other papers by Mr. S. Ramaseshan appearing in the symposium. Besides being the joint author of the introductory paper of the symposium, he has developed the ideas contained in it further and put forward a definite theory of the crystal forms of diamond, connecting them with the circumstances of the genesis of the diamond and with the surface energy of the molten carbon from which the crystals formed. The calculations made of the surface energy of different crystallographic planes in this connection suggested that diamond should exhibit various other cleavages besides the well-known octahedral one. This prediction has been verified in experiment.

C. V. RAMAN.

INTERNATIONAL CONTROL OF ATOMIC ENERGY

Atomic Scientists Memorandum to U.N.O.

Summary of Recommendations

THIS summary is taken verbatim from the Atomic Scientists Association's memorandum, which makes the following recommendations :—

1. That an attempt be made immediately to obtain an international agreement by which the use of atomic energy, the distribution of the essential raw materials for it, and the erection and operation of plants designed to produce or capable of producing active materials would be strictly controlled by the United Nations Organisation.

2. That this control be implemented by a system of inspection which would give inspectors appointed on behalf of U.N.O. the right of access to any place, plant or institution in any country for the purpose of ascertaining that there exist no sources of supply, plants or installations for atomic energy, other than those approved by U.N.O.

3. That all major sources of raw materials and all major production plants be handed over to U.N.O. and be operated (possibly by national contractors) under international management boards responsible to U.N.O. and guarded by men also responsible to, and appointed by U.N.O.

4. The United Nations Atomic Energy Commission should undertake the construction and operation of new large-scale plant for the production of fissionable material. These plants should be so distributed throughout the world as to ensure that if any nation should seize control of the plants operating in the area in which its own armed forces are predominant the remainder of the United Nations would jointly possess an overwhelming superiority in the production of fissionable material.

5. That the disposal of active materials produced in such plants and the research, development, and production of atomic explosives be reserved to U.N.O. and that any bombs made in that way, or the bombs made prior to the operation of this scheme, be kept in stores distributed throughout the world and operated as described under (3).

This does not imply that the signatories regard the atomic bomb as a desirable or suitable weapon for carrying out the policing functions of the U.N.O. In the present state of world apprehension, however, it seems necessary that atomic bombs should be produced and controlled by an international authority, to prevent any ill-disposed nation holding the threat of atomic warfare over the peace-loving nations of the world. When, however, the control authority is functioning effectively, it should be possible to envisage the cessation of the production of atomic weapons and the destruction of existing stocks. Atomic explosives could then be used for peaceful purposes only.

6. That as the scheme described above becomes effective, the existing secrecy rules be lifted, starting forthwith with the release of all basic scientific information, and that eventually all research and development be carried on freely and openly, with a duty to report to U.N.O. any significant results, which will, in general, also be published.

7. That the free movement and interchange of all scientists, including those working on atomic energy, be permitted and encouraged to the fullest extent.

8. In the implementing of the above proposals we are impressed with the feasibility of the recommendation made in the Acheson Report of the division of atomic energy activities into 'safe' and 'dangerous' activities and consider that an approach of this kind gives promise of an effective control of atomic energy developments together with a minimum encroachment of the national sovereign rights of the nations.

—(Courtesy of *Discovery*, June 1946, p. 175).

THE BATTLE OF STEEL

THE Government Control of an industry has something in common with the imposition of income-tax. Individually we are apt to find both irksome but collectively as a nation we must admit that they both seem justified by necessity. *The Battle of Steel*, a well illustrated booklet, published by the British Iron and Steel Federation, tells in a simple fashion how the iron and steel industry of Britain was harnessed and regulated during the six long war years.

Section 1 of the booklet, "Steel at War", opens with the statement that the conclusion of the Japanese war has made it possible to tell this story. It is not a story of new tonnage records—in fact the tonnage produced in 1937 was never exceeded during the war. This is accounted for by the cutting off of four-fifths of the raw material normally imported, black-out difficulties, and the transfer from the industry to the military forces of many young workers.

It was fortunate that the setting up of the Iron and Steel Control was aided by the existence of peace-time organisations—for the meeting of war needs proved to be no mean task. Even so there was no finality as the war needs were constantly changing.

The overshadowing problem was the utilisation of the poor grade ores mined in Britain instead of the usual rich imported material. All the skill of the management, steel workers and metallurgists was required to meet this situation. Arrangements had to be made for the railways to transport immense quantities of low grade ore from the mines or quarries to the steel works and at times this ore traffic had to be weighed against the more immediate needs of war—for instance during the preparations for the invasion of Western Europe.

In Section 2, "Struggle for Raw Materials" the dramatic story is told of how five ships broke through the German blockade of the Baltic—to bring much needed cargoes of iron and steel from Sweden to Britain. A later attempt, although less successful, brought cargoes which averted "bottlenecks" in the tank and aircraft output. Not only was it necessary to adapt steel and iron works for the use of native high phosphorus ores but metallurgists had to devise new steels so as to economise alloying materials in short supply. A national survey of scrap resources was conducted at the height of the war and was followed by the collection of disused railways, bridges, tramlines and even railings around parks and houses. The German bombing of Great Britain provided an additional 600,000 tons of scrap—about half of which came from London. A brief reference is made to the negotiations with foreign powers for the supply of special materials such as chrome and tungsten.

Section 3, "Transport", relates that although the quantity of finished steel carried was not excessive—the haulage of large amounts of low grade ore over long distances was a major problem for the railways. Extra hopper wagons had to be built to handle this traffic.

The concentration of shipping at relatively few ports on the west of the U.K. accentuated these difficulties. Further, owing to the exigencies of war, ships sometimes arrived unexpectedly at unusual ports and the controller had to decide whether the cargoes could be utilised locally or had to be carried by rail to the original destination.

"Brick without Straw" is the caption of Section 4 in which the metallurgist "comes to his own". When the tungsten supply from Burma was cut off by the Japanese invasion —three new types of high speed steel—all using less of this precious alloying element—were evolved.

Economies were effected by using tools merely tipped with high speed steel, by ensuring that high speed steel was used only for work for which it was essential and by a propaganda campaign insisting on the proper care of tools.

Special arrangements had to be made to meet the heavy demands for accurately dimensioned high tensile steel components of Bailey bridges required for the armed forces. The heat-treatment of unprecedented quantities of armour plate necessitated the commandeering of many large furnaces previously used in the enamelling trade—and the equipping of these with suitable heavy charging gear. Perhaps the least spectacular but at the same time the most important achievement was the work of the Technical Advisory Committee in reducing the 2,000 odd steel specifications of the British Standard Institution to less than 90—which nevertheless covered all war requirements. An immense amount of invention and adaptation helped to meet these war-time demands. Much of the work is still secret and it is expected greatly to benefit the British steel industry in future.

Section 5, "The Human Factor", emphasises the chief difficulties that the management and labour had to surmount, namely, the black-out, loss of the younger men to the Forces and the radical changes in production practice due to the raw material position. In spite of all ventilation arrangements the black-out screens made conditions very difficult for workers in the steel melting shops. During Air-raids skeleton crews remained on duty and submitted themselves to additional hazards to ensure that the steel in the furnaces should not be spoilt. Women were employed in large numbers—sometimes in works where none was to be found before the war.

The last Section, No. 6, entitled "Mulberries and Houses", refers to the steel requirements of the great prefabricated harbour which was established on the Normandy beaches and which made possible the large-scale invasion of the "Continent". These demands had to be met without interrupting the supplies for other war requirements. Turning from war to peace —mention is made of investigations which have already been carried out in the construction of steel-framed dwelling houses and the Section closes with the statement that the British steel industry believes that in post-war years its contribution to the advancement of the peoples' material welfare must be as vital as the part it has taken in securing Britain's Survival against Axis aggression.

FRANK ADCOCK.

SIGNIFICANCE OF HUMAN INDIVIDUALITY IN WORLD AFFAIRS

BEYOND THE FOUR FREEDOMS: 'I' AND SCIENCE

By J. J ASANA

(Gujarat College, Ahmedabad)

OUR civilization, of which science is such a distinctive feature, seems to have lost its moorings. Few thoughtful people will dispute the statement that mankind to-day is not happy and the existing situation in world affairs is most distressing. How to remedy this intolerable state of things is a problem in which scientific workers, too, along with other people are now vitally interested, as at the root of this problem lie the questions of knowledge and value, what is the true and what should be the 'good' course of action.

One of the most comprehensive theories, proposed during the last World War, to meet this very difficult situation is the theory of the four freedoms for the common man. A concerted planning of the world's resources on a world-wide scale has been envisaged to work towards and realize these freedoms; and it is commonly believed that scientific discoveries and inventions will play a leading part in humanity's march towards that goal.

The writer is one of those who feel that something more is needed to diagnose and remedy the existing unhappy situation in world affairs. This theory of the four freedoms recognizes, implicitly or explicitly, the significance of man the individual and aims at an all-round welfare of the common man. It may perhaps be elaborated in the direction of gaining further intellectual clarification and conviction with a view to formulating an ultimate purpose in the government and civilization of men. This may perhaps help us to see in a more clear perspective the intellectual conflict regarding the significance and welfare of the individual *versus* those of the community or the state,—a conflict of ideas, ideals and modes of governmental action, which, as all thoughtful people see, is now gaining a world-wide stage and which may be a prelude to another world-wide conflict of arms. It may be that the canker, which is undermining and destroying our civilization, may have something to do with the problem whether mind has any existence whatever independently of matter, whether it is not a queer phenomenon not dependent upon matter altogether and seeking some freedom from the tyranny of physicalism.

It is conceivable that this more or less hidden intellectual conflict may partly be responsible in delaying the formulation and adoption of an idealistic, philosophico-religious outlook not only on life but in public affairs also for the realization of universal peace and goodwill among men, which scientific workers along with other people so earnestly desire.

In this article are offered, primarily for the consideration of students of science and scientific workers, a few comments and reflections on a more or less recent trend in scientific observations and scientific thought. This is done with a view to discussing whether the so-called 'mind' (the thinking-feeling-willing 'I'), is merely a functioning or behaviouring of a biological organism, such as a human being, as assumed by the majority of men of science; or does it give any scientifically collected evidence of being an agency, an entity, more or less independent of the biological organism.

If scientific workers at the present time do not see, logically or illogically, any purpose or human values in their study of nature, including the body of man, this discussion is also intended to make an appeal to them that a time has come in world affairs in which men of science should make an attempt to examine more thoroughly the relation of mind and matter, or 'physicalism'. Many of us feel that it has a direct bearing on some of the live issues of our day and is no longer a question of academic importance between science on one hand and philosophy and religion on the other. Some of us feel that in view of the present turmoil in state affairs these questions perhaps demand greater attention than the advancement of science itself, at least for the present. And though men of science are also idealists in their way, in the pursuit of what they call truth, it may be worthwhile to look for some evidence in scientific thought itself to encourage those of us who feel inclined to join forces with what is best in philosophy and religion for formulating a purpose for civilization and thus to work for a purposeful progress of science inspired by an idealistic view of life.

To avoid any misunderstanding of the writer's attitude towards the value of science to this country, he may be permitted to strike a personal note at this stage of the discussion. He yields to none in his enthusiasm and devotion to scientific studies. As a teacher in science, with some experience of scientific research and some familiarity with the literature on rationalism over the past several years, he has grown to be convinced of the immense value of science to India. It can confer incalculable benefits on our people. And he is fully aware that many superstitions, going under the name of religion, have been doing great harm as obstacles to this country's progress towards light and liberation.

Is There an Irrepressible, Ever-Present 'I' in Man's Study of Nature Including His Body?

As all students of science know, many philosophers and religious-minded people for ages have been laying considerable emphasis on the significance of the human mind—the thinking-feeling-willing 'I'—in the study of nature and acquisition of knowledge. Men of science may have justifiably thought it expedient to put into background this wise admonition for some time. However, in view of the recent trend, described below, in different branches of science, it may be that the time has come for us, students of science, to give more attention

to the question of the relation of mind to matter, while not neglecting in any way our legitimate scientific studies.

PHYSICS AND 'I'

Scientific workers are familiar with recent advances in physics which lay considerable emphasis on the position of 'the observer', the human individual, in the study of the complex phenomenon of 'physicalism'. It is also well known that these remarkable researches have led many eminent physicists—Eddington, Jeans, Planck, Herbert Dingle and others—to make excursions into philosophy. Scientific workers with a philosophical bias may find recent communications of Professor Haldane and Professor Milne on "A Quantum Theory of the Origin of the Solar System" in *Nature*, No. 3927, February 3, 1945, highly suggestive. Professor Haldane suggests that the external source of energy postulated to make the sun emit the matter which condensed into the planets may have been a photon. In generating such a unique conception of photon, one wonders what part the phenomenon 'Haldane' plays.

EVOLUTIONARY BIOLOGY AND 'I'

The concept of the term 'progress' has been a matter of considerable controversy and is in fact the main thing at the back of the writer's mind in framing this article. In this connection it would be pertinent to our discussion to quote a few remarks of an eminent research worker in evolutionary biology, Professor Julian Huxley,[1] with regard to evolutionary progress. These remarks, it seems to the writer, have a bearing on the discussion whether mind, the so-called psychic content of human individuality, has any existence more or less independent of the biological organism, such as the body of man, with which it is usually associated.

Huxley[1] arrives at the following definition of evolutionary progress while discussing it in the final chapter of his recent book, *Evolution —the Modern Synthesis*, on page 564.

"We have thus arrived at a definition of evolutionary progress as consisting in a raising of the upper level of biological efficiency, this being defined as *increased control over and independence of environment* (Italics ours). As an alternative we might define it as a raising of the upper level of all-round functional efficiency and of harmony of internal adjustment". In other words, further evolutionary progress, further advance of the biological organism, lies in the direction of some state in which it gradually depends less and less on the environment, on physicalism, on the material conditions of life such as food, air, temperature, etc. This may mean that the advance of a biological organism, such as a human being, lies in getting independent of—if control means some independence—its biological or physiological activities, because, one wonders, what are physiological activities, the process of living, without environmental conditions. Is it not difficult to conceive of a biological organism, which is in a sense so much environment itself, getting increased control over the environment, that is, partly over itself, without the help of some energy, some agency on which the organism is dependent or with which it is associated? If the advance of the biological organism, such as a human being lies in getting independent of its environment, of its ecological conditions, one wonders, wherein lies the next field of evolutionary progress, the field for further advance.

Taking into consideration this scientific criterion of evolutionary progress it seems reasonable to support the inference of the philosopher that mind, 'I', may be an agency, an entity existing in its own right also. The investigation and understanding of its nature may be the next field of advance, of evolutionary progress. Hence our appeal for the recognition of the significance of human mind in world affairs.

It is true that we have not yet obtained any adequate conception of such a nature of mind. Could this elusive agency, 'I' be the element on which *Yogis* and many of the great sages of India have laid so much stress, towards which they have directed their gaze and which probably they attempt to realize in some of their most difficult practices?

PHYSICAL RESEARCH AND 'I'

As said above, the biological or the physiological concept of mind is that it is merely a functioning of the sense and motor organs in connection with the central nervous system of the biological organism. It is further believed by almost all men of science that this behaviouring is subservient to and lies within the operation of the scientific laws of physicalism as known to-day. In short, the attributes of the mind are the attributes of the organism and no more.

Scientific workers would, therefore, naturally demand some evidence, obtained by scientific methods of enquiry, to induce them to give further consideration to the alleged extra-corporeal existence of the psychic content or mind of the organism. Are there any queer, unusual, extraphysical attributes of mind, which will bear the weight of scientific analysis and criticism?

A. Extra-Sensory Perception (ESP)—

Biological organisms, such as human beings, perceive or see with the aid of eyes and thus gain knowledge of the events taking place in their environment. About sixty-four to sixty-five years ago, scientific investigations[2] were undertaken with a view to finding out whether other modes of perception occur and whether 'knowledge of things is acquired by a person in whatever manner, without the use of the ordinary channels of sense-perception, of logical inference, or of memory'. It was also their aim to learn more about such phenomena as are designated by the terms 'mesmeric', 'psychical', and 'spiritualistic'. These investigations took the name of psychical research.[3]

About the year 1930 Dr. J. B. Rhine[5] and his collaborators started a series of experiments at Duke University, North Carolina, U.S.A., to investigate the following problem:

"Is it possible repeatedly to obtain results that are statistically significant when subjects are tested for knowledge of (or reaction to) external stimulii (unknown and uninferable to the subject) under conditions that safely exclude the recognized sensory process?"

At the time of writing, many reports[4] of scientific researches, conducted over long periods of time, to find out whether an extra-sensory mode of perception (ESP) is experimentally demonstrable, have appeared. Several authoritative books[5] have been published reviewing scientific evidence to show that ESP and kindred psychical phenomena such as telepathy, clairvoyance, precognition, retrocognition, etc., occur. Rhine[7] gave in June 1944 a short review of the progress and status of research on ESP during the preceding ten years or so. He says, "A greater number of important investigations of ESP occurred in this ten-year period than all the previous history that covers at least fifty years. Most significant of all the evidences of expansion is the extent to which the investigation of ESP has invaded the collegiate laboratory of psychology and involved the participation of psychologists themselves. ESP capacity was found among a wide range of subjects tested —male and female, children and adults, blind and seeing normal and abnormal". He adds, "But with regard to the physical world, the ESP process showed distinct peculiarities in every respect. In contrast to its relation to sensory perception, *physical law seemed to play no part in the process of extra-sensory perception* (Italics ours). Whereas visual stimuli are less intense, the further away from the eye, ESP success did not fall off with the removal of stimulus to greater distances. Other physical conditions too, such as the angle at which the object was placed or the interposition of the barriers, had no influcnce on the results. ESP was found to be unaffected by the spatial conditions imposed."

B. Psychokinesis (telekinesis) PK—

Rhine and his associates[4] have also undertaken to investigate experimentally whether mind can act directly upon matter, whether mind has power to exert its influence on the physical world without any recognized intermediate agencies. In February 1934 experiments were devised in the Parapsychology Laboratory of Duke University, America, to investigate, as Rhine[6] says, the claims of the gambler that the state of mind can influence the fall of dice. He came to the following conclusion in 1944, after ten years of these dice-throwing studies:—

"*There is a direct psychical effect exerted on the fall of the dice.* It is one of the most, perhaps *the* most, demonstrable of the phenomena of parapsychology. It is a psycho-physical effect which is kinetic in its result and may be termed *psychokinesis*, or PK. This effect may be fundamentally the same as the casual action of the mind on the brain, and psychokinesis need not necessarily involve distance. Names are of little importance, but this explains one of the main objections to the more familiar term *telekinesis* (movement at a distance)."

Some Reflections

Some of the results of the researches on "ESP-PK-process complex" seem to indicate that a biological organism, such as man, can perform some functions which at present cannot be explained on physiological principles, which are in their turn dependent upon our existing concepts of space, time and matter. They seem to be in certain respects *extra-physical* and *extrabiological*. From the evidence presented by psychical research, one may infer that these functions may be some unique attributes of the assumed entity called mind; these functions may bear witness to its existence, an existence more or less independent of the biological organism. These researches lend point to what Tyrrell[5] calls "this 'I-principle' this inscrutable something which we know only in the unique and unanalysable experience of being 'I'."

How are such academic discussions to help us in preventing men from quarrelling and inflicting great hardship, pain and suffering upon one another, where scientific discoveries and inventions too are pressed into service?

For ages past prophets, philosophers and many religious-minded people, desiring to see universal peace and harmony among men, have been extolling the dignity of man, the individual. They have laid stress on the spiritual part of the nature of man and have been constantly making an appeal for its recognition in the affairs of life. We know the wise admonition to man, to see his self in the other self, to be the promoter and guardian of his neighbour's welfare also.

Apart from sentiments and sympathy, do students of science see any reason, based on scientific evidence and on intellectual grounds, to agree with the statement that there may be some truth in this philosophic contention and religious appeal? In view of the growing influence and prestige of science in world affairs, do scientific workers, on the considerations set forth here, feel inclined to have the individual as the centre of interest in advocating a world policy for the government of men?

It may be that the 'I' stands behind the four freedoms. In our efforts for its spiritual realization, we see no intellectual compunction for science to hold back. And may we hope that the land that has given us the *Gita*, the land whose ancient philosophy, religion and culture have contributed so largely to the formulation of the ideal of 'self-realization' making it the supreme purpose of life, may produce many '*karma-yogis*' in science, not only to work for the material welfare of their country, but also to help philosophy and religion in making mankind see the error of its ways and in turning its gaze towards the significance and profundity of 'the I-principle'.

---

1. Huxley, J. S., *Evolution—the Modern Synthesis*, 1942. 2. *Society for Psychical Research*, London, "Objects of the Society" *Proc. S.P.R.I.*, 1882, 3–6. 3. *(a) The American Society for Psychical Research*, Inc. 40 East 34th Street, New York. *(b) Boston Society for Psychic Research*, Inc. 719, Bolyston Street, Boston, Mass. *(c) The Institute Metapsychique*, France. 4. *Journal of Parapsychology*:—Duke University Press, Durham, North Carolina, U.S.A. 5. *(a)* Rhine *et al.*, J. B., *Extra-sensory Perception after Sixty Years*, 1940, Henry Holt & Company. *(b)* Tyrrell, G. N. M., *Science and Psychical Phenomena*, 1938, Harper & Brothers. *(c)* Carnington, W., *Telepathy, an Outline of its Facts, Theory and Implications*, 1945, Methuen & Co. 6. Rhine, J. B., *Journal of the Am. Soci. for Psy. Res.*, 1944, **38**, 4, October. 7. —, *The Jour. of Parapsy*, 1944, **8**, 2, June.

PLOT SIZE IN YIELD SURVEYS ON COTTON

By V. G. PANSE

(Institute of Plant Industry, Indore)

AN important feature of province-wide yield surveys on cotton,[1] wheat[2] and rice[3] conducted by the Indian Central Cotton Committee and the Imperial Council of Agricultural Research is the large size of the sample plot, ranging from 1/60 to 1/10 acre. Workers in England[4] and America[5] have on the other hand concentrated on small plots of 10 or 12 sq. ft., and have developed methods of sampling conditioned by the scarcity of rural labour but excellent transport facilities and easy access by motorable roads to all parts of the tract to be sampled. Hubback[6] in the first sample survey ever carried out in India also used a small plot of 13·6 sq. ft. and recently Mahalanobis[7] adopted the plot size (actually 12·5 sq. ft.) together with Hubback's method of marking the plot by means of a triangular frame in yield surveys on wheat and gram in two districts of Bihar Province.

The question of plot size in Indian crop surveys has lately become a subject of considerable controversy.[8] The alternatives are the large plots employed in the I.C.C.C. and I.C.A.R. surveys and the small plots recommended by Mahalanobis. Crop cutting experiments on large plots are annually conducted by the Government departments concerned with the forecasting and estimation of crop yields in the temporarily settled provinces in British India and their choice of large plot is well justified on practical grounds. Sukhatme[9,10] has now published results on a more serious aspect of the comparison between different plot sizes and if his findings are found generally applicable, small plots must be rejected as giving highly biassed and, therefore, misleading estimates of yield. He showed that the use of small plots, ranging from 12·6 to 118 sq. ft., led to an overestimation of yield from 5 to 49 per cent. as compared to a plot size of 472 sq. ft. employed in the I.C.A.R. surveys, the smallest plot size giving the largest bias. An experiment on this problem was carried out on cotton in 1945-46 and the results are briefly described below.

The experiment was planned in two sections. The first was located at Indore in the fields of the Institute of Plant Industry and in four neighbouring villages. In all 26 fields were selected. The second section of the experiment was carried out at Government Farms in C.P. and Berar through the kindness of the provincial agricultural department. Here also there were 26 experimental fields distributed among seven Farms.

In earlier investigations[11,12] it had been established that precision of the results was not materially affected by changing the plot size from 1/20 acre to 3/10 acre. In the present experiment 1/20 acre was, therefore, adopted as the standard plot size with which it was proposed to compare two smaller sizes, 1/200 and 1/2000 acre. In each selected field two plots of each size or six plots in all were marked randomly according to the procedure of random selection employed in the cotton survey. Cotton in Central India and C.P. is sown evenly in rows with a row spacing of about 14" and 18", and unlike cereals and other crops which are either broadcast or sown in irregular lines in many parts of the country, plot area in cotton consists of a specified number of rows of a given length. Plot size in sample surveys on drill-sown crops in America[5] is similarly defined as a certain number of contiguous rows of a fixed length. In the present experiment, plots of 24 rows 78 ft. long, 8 rows 23 ft. long and 3 rows 6 ft. long, at Indore, and of 22 rows 66 ft. long, 7 rows 21 ft. long and 2 rows 7 ft. long in C.P., were marked as representing plot sizes of 1/20, 1/200 and 1/2000 acre respectively on the assumption of the above row spacings. After the pegs had been fixed at the four corners of a plot in the middle of the inter-row space, exact dimensions were measured in feet and inches and the actual size of the plot was calculated. The following were the average plot sizes obtained:—

Model plot size	*Actual plot size* Indore	C.P.
1/20 or ·05 acre	·056 acre	·054 acre
1/200 or ·005 acre	·0055 acre	·0054 acre
1/2000 or ·0005 acre	·00053 acre	·00053 acre

Actual plots were thus slightly larger than the corresponding model sizes owing to the distance between rows being a little wider than was assumed. The results of the investigation are strictly applicable to the actual plot sizes; but in the discussion that follows it is convenient to refer only to the model sizes to which the plots approximate.

There were 3 to 6 pickings in the plots at Indore and 3 to 5 pickings in C.P. The yield of each plot was converted into pounds of seed-cotton per acre by using the appropriate area factor before results were analysed statistically.

Average yield per acre estimated from the three plot sizes is shown below:—

Plot size	*Average yield lb. per acre* Indore	C.P.
1/20 acre	195·7	322·2
1/200 acre	197·0	354·9
1/2000 acre	221·3	421·8

There was a gradual increase in yield per acre as the plot size was increased in the C.P. experiment. At Indore also, the yield per acre from the smallest plot was larger than the other two plots which between themselves did not show any difference. A statistical comparison of the differences gave the following results:—

Plot size comparison	*Difference lb. per acre with s.e.* Indore	C.P.
1/200—1/20 acre	1·3 + 21·6	32· ± 20·7
1/2000—1/20 acre	25·6 ± 25·2	99·6 ±
1/2000—1/200 acre	24·3 ± 23·3	67· 0 ± 58·3

None of the differences was significant; but the excess of yield estimated from 1/2000 acre plots over 1/20 acre plots in C.P. was almost twice its standard error and approached significance on the 5 per cent. level. There is ground for suspicion here that small plots, particularly of 1/2000 acre, overestimate yield. Overestimation in such plots has been established by Sukhatme in wheat and rice and the present results are in the same direction.

The comparison of yield estimated from sample plots with the yield obtained by harvesting the whole field is the ultimate criterion for judging whether the sample plots truly represent the fields. Complete yield data could not unfortunately be collected for fields at Indore as in certain cases the owners mixed the yield of two or more fields and two or three fields were damaged by cattle before their final picking was over. In the C.P. Farms, however, correct yield figures were recorded for all fields included in the experiment and their comparison with estimates from sample plots gave the following results:—

Comparison of plots with fields	*Difference in yield lb. per acre with s.e.*
1/20 acre plot—whole field	$26{\cdot}7 \pm 26{\cdot}5$
1/200 acre plot—whole field	$59{\cdot}4 \pm 28{\cdot}1$
1/2000 acre plot—whole field	$126{\cdot}3 \pm 57{\cdot}6$

The yield estimated from 1/20 acre plots agreed quite well with the yield for the whole field as the difference between the two was no more than its standard error; but the excess of the yield estimated from the other two plots over the yield from the whole field was greater than twice its standard error and clearly significant. This result provides evidence that plots of 1/200 acre size or less overestimate yield. With 1/2000 acre plots the overestimation was as high as 42·7 per cent. of the true yield.

The relative efficiency of plots of different sizes was studied though this point is of little practical interest owing to the biassed estimates that smaller plots give; but setting this consideration aside, the number of plots and fields required to be sampled to estimate yield with a standard error of 5 per cent. was calculated separately for Indore and C.P. from the observed values of intra-field and inter-field variability. From C.P. results, plots of 1/20, and 1/200 acre size did not appear to differ in efficiency, but five plots of 1/2000 acre were equivalent to a single 1/20 acre plot, i.e., gave a result with the same precision, the number of fields being kept constant. At Indore both smaller plots had a much lower efficiency than the standard plot, and with 1/2000 acre plots even ten plots per field could not give an estimate with the same accuracy as provided by a single plot of 1/20 acre per field.

As far as the yield surveys on cotton are concerned the present results are limited to showing that the plot size of 1/10 acre adopted in these surveys (or 1/20 acre recommended for irrigated cotton) is well beyond the range of influence of border bias exhibited by small plots; but even if small plots were free from bias, it is improbable that they would be considered suitable in practice. Unlike cereals, cotton is harvested in several rounds of pickings and consequently the plot must be maintained intact in the field for 3 or 4 months and visited repeatedly. The produce of each picking should be sufficient in quantity for accurate weighment by ordinary balances. The dimensions of the cotton plot have to be fixed on these and other similar considerations. The important conclusion emerging from the present experiment is that small plots give biassed estimates of yield not only in broadcast or unevenly sown crops as shown by Sukhatme, but also in drill-sown crops with evenly spaced rows. While small plots used by English and American workers may not be open to serious objection for comparative purposes, the possibility that the yield estimates derived from such plots are seriously biassed needs a careful examination.

In his report on the Bihar crop survey[7] Mahalanobis considered the plot size of 12·5 sq. ft. marked by a triangular frame to be efficient, economical and convenient. It is interesting to note, however, that in a later article[8] he has recommended a circular plot of 50 to 100 sq. ft. without giving any experimental data or precise reasons in support of this change; but Sukhatme's results have shown that the the latter plot size is also not free from bias. In the present experiment even a plot size of 1/200 acre or 219 sq. feet was found to overestimate yield. It is noteworthy that Sukhatme had tried circular plots marked by an apparatus very similar to that described by Mahalanobis, but his conclusion was that for a given plot size it made little difference in the magnitude of bias whether the plot was triangular or circular. The new plot size proposed by Mahalanobis would, moreover, be difficult for handling by the travelling investigator without the help of hired labour and would thus be deprived of the special advantage claimed for the smaller plots while retaining their defect in giving biassed estimates. On the experimental evidence discussed above, only large plots can be considered to be free from this serious drawback.

The experiment was financed by the Indian Central Cotton Committee.

1. Panse, V. G., Kalamkar, R. J., and Shaligram, G. C., *Curr. Sci.*, 1945, **14**, 287–291. 2. Sukhatme, P. V., *Proc. Ind. Acad. Sci.*, 1945, **21**, 328–341. 3. —, Reports on crop cutting experimental surveys in Tanjore, Raipur and Kolaba, 1945, *Imp. Council Agri. Res.* 4. Cochran, W. G., *Jour. Amer. Stat. Assoc.*, 1939, **34**, 492–510. 5. King, A. J., McCarty, D. E., and McPeek, M., *Tech. Bull.*, No. 814. 1942, U.S. Dept. *Agri.* 6. Hubback, J., *Agric. Res. Inst., Pusa. Bull.*, 1927, **166**, 166–175 7. Mahalanobis, P. C., *Sankhya*, 1945, **7**, 29–106. 8. —, *Ibid.*, 1946, **7**, 269–280. 9. Sukhatme, P. V., *Curr. Sci.*, 1946, **15**, 119–120. 10. —, *Nature*, 1946, **157**, 630. 11. Panse, V. G., and Kalamkar, R. J., *Curr. Sci.*, 1944, **13**, 120–124. 12. —, *Ibid.*, 1944, **13**, 223–25.

THE CONCEPT OF VESTIGIAL ORGANS AND THE VASCULAR CRYPTOGAMS

By Dr. T. S. MAHABALE

(Gujarat College, Ahmedabad)

THE body of a plant is a complex organization made up of parts working together towards the common goal of maintaining its life. Some of these parts are concerned with the vital functions of absorption, assimilation and reproduction and others are there merely to help these. A few others do not undergo full development and are in no way concerned with the vital physiological processes and as such are quite useless. Such imperfectly developed parts in the body of an organism, in no way concerned with vital functions, "bearing a plain stamp of inutility" are called the vestigial organs. They are known to occur in many plants and are inherited with the same regularity with which the vital ones are inherited. But being in no way concerned with the essential process in the plant-life they seldom undergo modifications as do the other parts such as leaf or stem in response to the external stimuli acting on them. In fact they behave in this respect, in a manner quite opposite to that of the latter. Whereas with the passage of time the essential organs get modified according to the conditions under which they live, these organs get arrested or degenerated. Despite this fact they preserve their original character, though in a reduced form, in a very remarkable manner, and in course of time become "the unperished symbols of great antiquity". Viewed in this light the body of a plant is a bundle of some useful and some useless parts.

The form of such antic parts is often very simple, and therefore, systematists find it easy to trace their homology. In doing so they safely disregard the physiologist's warning that they are quite useless or ignore the anatomist's conviction that they are too simple, and use them in phylogenetic speculations. The number of such useless parts in the body of a plant is generally very small, although in stray instances it is so large as to give an impression that the whole body of the organism is a museum of such relict parts, *e.g.*, that of King-crab, *Limulus* in animals or that of *Welwitzia* in plants in which practically every degenerate part has some past history to proclaim.

As pointed out by Darwin[1] (1888), according to the earlier theories of 'Genesis' such parts found no satisfactory explanation. They were supposed to be present in the body of an organism either "to complete the scheme of Nature" or to "keep up the relations of symmetry" which they often did not. But according to Darwin's theory of descent through variations and Natural selection, they afforded another line of positive evidence in support of his theory. Ernst Hækel[2] (1886), Darwin[3] (1888), Goebel[4] (1900), Bower,[5,6] (1901, 1935), Jeffrey[7] (1917), Sahni[8,9] (1923, 1925), Browne[10] (1927) and others have used these structures in support of their views on certain problems in plant morphology; but on the whole, attempts at presenting them in a connected manner have been very few in botanical literature. The reasons for this are not far to seek. The examples of such wasteful heritage are far more numerous in animals than in plants. For example, in the body of man alone, anatomists have enumerated a few hundreds of such vestigial organs such as the rudimentary stalk of the pineal body in his brain, the nictitating membrane in the eye, seven useless muscles never called upon to move his ears, and a good many others in the neck to hold his head down, the embryonic tail of commensurable dimensions in the third week of conception, not to mention the vermiform appendage, wisdom teeth cut at later age, etc. A plant has no such array of useless parts. The fixity of life means a rigid discipline. It means no scope for ambulation, less scope for speciation, and hence for acquiring new organs to master the new haunts of life. Whatever organs it has, have to be used to the best possible advantage, and that too if possible for purposes more than one. A plant, therefore, can hardly be extravagant or over-exuberant in its expression of parts, much less in the expression of its useless parts. Secondly, by far the most important process in the life of the plant is the process of photosynthesis. The law of surface expansion is very important here. More the surface exposed, better it is for the plant; and to do that effectively many plants develop a diffuse form quickly. In attaining it some of the developmental stages are simply passed over in ontogeny and others are deleted. In doing so, however, each one of the innumerable growing points of a plant repeats the story of the development of the embryonic shoot on a miniature scale, and to quote Sahni (1925, p. 204), "in both space and time". Generally there is a tendency to delete obsolete parts: and the parts that succeed show a serial homology. For example, the first embryonic leaf is generally simple in the sporelings of many ferns such as *Osmunda* or *Marsilia* but becomes compound later after having passed through a series of transitional forms (see Bower,[11] 1923, pp. 87 and 93). In the sporelings of *Botrychium virginianum* and *Helminthostachys zeylanica*, however, the cotyledon is compound and trifoliate *ab initio*. The seedlings of some other ferns also omit the bifoliate stage like *Botrychium* and *Helminthostachys*. But despite such examples of rapid development involving the deletion of parts one is surprised to find that the vestigial organs occur in almost all the groups of plants, *e.g.*, the auxiliary cells in some red algæ, the suffultory cells in some species of *Bulbochæte*, the spermatia in many rust fungi, the paraphyses in mosses, the amphigastria, perigynia and perianth in some liverworts, the annulus in equiseta, the ligule in lycopods and selaginella, the aphebieæ in certain ferns, the stigma

in the flowers of *Welwitzia*, the papillæ representing a stamen or a pistil in flowers of angiosperms, the suppressed flowers in the spikelets of grasses, etc. These will suffice to show that the formation of vestigial organs is a phenomenon of very wide occurrence throughout the rubric of the plant kingdom. The present paper, however, deals with the vestigial organs of the vascular cryptogams mainly, as these plants constitute a compact and archaic group possessing some striking examples of such organs.

SOME CHARACTERISTICS OF THE VESTIGIAL ORGANS

From what has been said above, it will be seen that the two important features of the vestigial organs are the simplicity of their structure and uselessness. The former is often quite obvious, but the latter is not easy to prove in many cases. Theoretically it is easy to conceive of some organs as useful and others as useless; but even then it is necessary to remember that those organs that are supposed to be useless now, are believed to have been useful in some remote past in the ancestors of plants which possess them to-day. In other words, their present vestigial state only indicates the negative extreme to which they have been driven from a state of usefulness through the operation of time and environment: and, therefore, it should always be possible to find several intergradations between these two extremes at any time in the history of the plant kingdom. And so it is. There are thoroughly vestigial organs, dwindling organs, arrested or aborted organs, atrofied organs, conservative yet simple organs, rudimentary organs, nascent organs, ephemeral organs, a long series of more or less useless organs. In each one of these the emphasis is not only on their simple character but also on their doubtful utility; and to reach a level of uselessness from one of utility, there must have been a long process, involving in the majority of cases millions and millions of years, if not billions. At any rate a consideration of the element of time in the history of these organs is of paramount importance as it is undoubtedly so in the case of those structures with which a palæobotanist deals. When this fact is realized much of the controversy over the relative importance of these organs in morphological discussions loses its sting; as in one's eagerness to prove that a particular organ is vestigial in such and such a group, one is likely to emphasise either its simplicity or its inutility or its antiquity. But such a unilateral emphasis on any one of these features is likely to lead to erroneous conclusions. What matters most in their consideration is probably the relative level of vestigiation shown by the organ concerned at any particular stage in the history of the plant both in the cycles of ontogeny as well as phylogeny. Thus an organ may develop only in embryological condition and may become vestigial in adult state. Some other organs may persist in adult condition but only in an arrested or atrophied form. Still others may remain nascent for all the time to come under normal conditions but under certain abnormal conditions may resume their original form. A few others may adapt themselves to some secondary function and look quite odd, *e.g.*, a root-like organ serving as a stem, *e.g.*, protocorm of *Lycopodium*, or a stem-like organ serving as a root, e.g., the rhizophore of *Selaginella*. From the phylogenetic point of view also, these must be carefully considered, as their occurrence is sometimes confined to the limits of individual plant, sometimes to those of a class. When they occur in two allied groups, we get good examples of recapitulation. Their testimony in phylogeny, therefore, has to be taken with a certain amount of scepticism, as the same organ may appear repeatedly in different periods in the past history of the same group; or it may occur polyphyletically in different groups at the same or different periods; and having lost its function, it may be reduced to the same level of vestige through a series of similar changes. The recognition of this fact is of great importance in dealing with them as many of our present-day series of plants are reduction series. And hence Goebel (1900, p. 61) warns :

> "Arrested organs may be such as generally in the existing species (or in its one sex) never reached complete development; it is only our synthetic necessity which forces us always to the assumption of reduction-series, of which, however, many can only claim to be fictions, imparting æsthetic pleasure of bringing a series of facts into connection with one another."

Keeping this precaution in mind we shall now make a brief survey of the vestigial organs of the vascular cryptogams.

A SURVEY OF THE VESTIGIAL ORGANS OF THE VASCULAR CRYPTOGAMS

For the sake of convenience these organs will be discussed as under :—

(1) *Embryonic organs* which become vestigial in later stages;
(2) *Rudimentary organs* which never attain full growth under normal conditions of life but under exceptional conditions reveal their true nature;
(3) *Arrested* or *atrophied organs* which persist in a simple form in adult configuration; and
(4) Internal organs reduced to vestigial state; the *vestigial tissues*.

(1) *Embryonic organs.*—The true nature of the embryonic organs is often difficult to decide as they are ephemeral and as such represent only decadent stages in the early development of a plant. Secondly, whenever an embryonic organ is said to be vestigial, it is with reference to a particular cycle of ontogeny. It may or may not be vestigial in the life-cycle of another plant belonging to the same or another group. Three embryonic organs have been recognized as such. They are : (i) Foot, (ii) Suspensor and (iii) Protocorm.

(i) *Foot.*—It is well known that the root is a secondary organ of absorption of a leafy sporophyte, the primary organ of absorption being foot. The main function of the foot is to absorb food material from the tissues of the gametophyte till the young sporophyte developing on it is able to absorb food material for itself from the soil with the help of the root, when present. It is generally formed from the hypobasal half of the two-celled embryo and takes a position diametri-

cally opposite to that of the leaf in the quadrant stage. This possibly suggests that the leaf and foot have opposite polarities like those of root and stem. And, therefore, what root is to the adult sporophyte, the foot is to the young plant. But in further development of the plant as the root and shoot assume greater proportions, the foot dwindles and is left as a small vestige completely lost in the adult stage.

That foot is an organ of considerable importance was known long since, as it happens to be the only absorbing organ of the non-leafy sporogonia of the mosses and liverworts. In the Anthocerotes also it becomes an organ of great physiological significance. In the sporophyte of this group, *Notothylas* excepted, there is a basal meristem which gives rise to a series of tetrads of spores from below. These ripen acropetally and are dispersed by the hygroscopic movements of the columella and elaters through the valves of the sporogonium. On account of the basal meristem the sporophyte has an infinite capacity for growth : and to nourish such a growing sporophyte the foot becomes very massive and a permanent structure in the morphology of the sporophyte. The foot, therefore, in this group is no longer an ephemeral organ as in the majority of liverworts but an organ of considerable utility throughout the life of the sporophyte. In the great majority of ferns also, the foot is only an embryonic organ but in the embryos of *Marattia douglassi*, *Kaulfussia æsculifolia* and *Equisetum debile* it is a massive organ which persists much longer than it does in other plants of these groups. It was in consideration of such facts in the embryogeny of the Anthocerotes and the Eusporangiatæ that Campbell[12] (1911, p. 211) stated, now more than thirty-five years back : "Indeed so marked are the resemblances in the early stage of development that they make the inference almost irresistible that the Ophioglossaceæ must have descended from some simpler forms whose sporophyte bore a strong resemblance to *Anthoceros*." As a matter of fact the embryo of *Anthoceros* bears such a close resemblance to the embryo of *O. moluccanum* or to the adult plant of *O. simplex* that one is tempted to call *Anthoceros* almost a pro-*Ophioglossum*. The significance of Campbell's inference, however, became apparent only after the discovery of the Psilophytales from the Rhynian cherts by Kidston and Lang[13] (1917-1921) and after the discovery of the gametophytes of the Psilotaceæ by Holloway[14] (1917) and Lawson[15] (1917) in 1917. In the embryogeny of the Psilotaceæ, there is no root, the whole of the lower part of the embryo being considered to be foot. Curiously enough this foot of the Psilotaceæ bears a close resemblance to the foot of *Anthoceros* and is as much prominent in the embryo of *Tmesipteris* as it is in *Anthoceros*. Nay, at a certain stage in the embryogeny, the whole of the embryo of *Tmesipteris* is considered to be all foot by Holloway (1917). In the early life of the plant, therefore, the foot is a very useful organ in *Psilotum*, *Tmesipteris* and *Anthoceros*. This was a striking confirmation of the earlier idea that even in the early vascular plants the foot must have been a very useful organ and not only vestigial as in the later vascular plants.

In the examples mentioned above the foot is a useful organ for a long time in the life of the plant. Quite an opposite of this is seen in the endoscopic embryos of *Selaginella* and *Lycopodium*. In these plants the foot becomes vestigial at a very early stage in the embryogeny. In *Selaginella* the embryo being endosporic, it is nourished by the parent-plant. The gametophyte becomes consequently reduced and the food material is stored in the lower part of gametophyte in the form of a frothy mass. The suspensor pushes the embryo in this region, and the foot, therefore, is reduced to a vestigial state soon. Between these two extreme cases (1) where the foot is an extremely useful and persistent organ and (2) where it is a mere decadent useless stage, in the great majority of the vascular cryptogams, it is an ephemeral organ which becomes vestigial in adult condition.

The other embryonic organ which becomes vestigial in adult state is suspensor. Its main function is to change the direction of the growing embryo in such a manner as to push it in those regions where the food material is stored. It is a matter of common knowledge that this organ occurred repeatedly in different groups of plants in the history of the plant kingdom, *e.g.*, in the Lycopodiales, Filicales, Coniferales, Gnetales and in angiosperms. Its phyletic history shows that it is not of constant occurrence even within the limits of a genus, much less in larger groups such as classes or phyla. For example, it occurs in *Botrychium*, *Helminthostachys*, *Danea*, and *Angiopteris*; but it is not of quite constant occurrence in the first and the last genera mentioned above. Once Miss Lyon[16] (1915) was so much impressed by this structure that she actually proposed a new genus "*Sceptridium*" in the Ophioglossaceæ to include such species of *Botrychium* as have suspensor. It occurs in almost all the species of *Selaginella*, but it is said to be absent in *S. pumila* found in Cape Colony, South Africa (see Duthie,[17] 1926). Obviously then in suspensor we are having a decadent structure not of constant occurrence in phylogeny. According to La Motte[18] (1937) in *Isoetes* also it shows a great variability of direction.

As a rule the development of such decadent parts is very rapid in ontogeny and they disappear also very rapidly in the life-cycle : because, the parts that serve no useful purpose in the economy of a plant are last to appear and first to disappear, *e.g.*, the corolla in Cruciferæ or the calyx in Compositæ. Very often these organs show precocious development and on that account look very conspicuous in comparison to the surrounding parts at certain stages in embryogeny. Thus the suspensor of *Angiopteris* is the largest part of the two-celled embryo, and so is the foot in the early embryos of *Tmesipteris*, *Marsilia*, *Equisetum*, etc.

The third embryonic organ of the vascular cryptogams which becomes vestigial later is the protocorm. The students of cryptogamic botany are familiar with the classical theory of protocorm as the forerunner of the vascular sporophyte enunciated by Treub[19] (1884-1888). This organ develops as a massive structure in lycopods, having all the characteristics of a

shoot. It closely resembles the adult plant of *Phylloglossum* with its tuber and annual cluster of leaves as in some orchids such as *Habinaria diphylla*. To this embryonic tuber-bearing protophylls Treub has given the name "protocorm". He considered it to be an organ of great antiquity. But his claim was freely contested by Goebel[20] (1904) and Bower[21] (1908) who looked upon it as an organ of perennation having only physiological significance. Holloway[22] (1917-1920), however, in his researches on the prothalli of the New Zealand species of the genus *Lycopodium* found that this organ though of great use to the plant in perennation, is capable of dividing dichotomously and can bear bulbils as are found on the sporophyte of *Lycopodium*. And, therefore, he came to the conclusion that it is not an organ of mere physiological importance but also of phylogenetic significance.

But the most unexpected confirmation of Treub's views came with the discovery of the Psilophytales by Kidston and Lang (1917). In the morphology of these of primitive land plants, there is a swollen portion at the base, and especially in *Hornea lignieri*, which bears a close resemblance to the protocorm of lycopods. This means that we are able to telescope the existence of the protocorm of modern lycopods in the Devonian plants, a period of not less than 300 millions of years. What is then the real significance of this organ? Was not Treub[23] (1890) right in regarding it as a vestigial structure of great antiquity? Probably he was. Because, it is quite possible that this structure might have arisen as an organ *sui generis* which might have had in some remote past adapted itself secondarily to its present function later, for which it was not very well suited. And hence in course of time it may have become partly vestigial and partly useful as Holloway[24] (1920, p. 233) thinks. Such examples of secondary adaptations of vestigial organs are quoted by Darwin[25] (1888) himself. For example, the styles in the flowers of some Compositæ though vestigial for their original purpose, secondarily help to brush aside the pollen. This is really a good example of successful secondary adaptation on the part of a vestigial organ, whereas protocorm and rhizophore suggest imperfect secondary adaptation. To my mind the same is probably the explanation of the axes of intermediate character such as Nathorstiana stem base, rhizomorph of *Isoetes*, Stigmarin axes, rhizophore of *Selaginella*, etc.

Another good example of successful adaptation on the part of a reduced structure is to be found in the hydathodes of *Equisetum*.[26] It is generally believed that the Calamites had leaves larger than those of the modern *Equisetum*. The former had stomata on the adaxial surface which became useless later; but subsequently they got associated with a vein and were transformed into hydathodes of epithem-type, and are functioning as such in many species of living *Equisetum*.

(2) *Rudimentary or Nascent Organs.*—Apart from the vestigial organs noticeable in embryogeny, a rudimentary organ may persist to a much later stage in a nascent form. Ordinarily it does not reveal its vestigial nature; but under the strain of some abnormal conditions it is brought out very clearly. Two such examples at least are known. In some species of *Selaginella* there are small pads or protuberances in the axils of leaves in the place of rhizophores, *e.g.*, in *S. rupestris*. These generally remain localised, but under the exceptional conditions they grow out into normal rhizophores and bear even leafy shoots. The other example is to be found in the annulus of *Equisetum*. In the majority of species of *Equisetum*, the annulus does not bear any sporangia; but the annulus of *E. giganteum* and *E. prœaletum* is normally sporangiferous. What is more interesting, some species such as *E. palustre*, or *E. arvense* show this condition occasionally.

Many of the abnormalities interpreted as reversion to the ancestral condition are probably due to the fact that the organs in question have been retained in course of evolution in a very reduced form. It is on this hypothesis that Bower (1901) considers the abortive sporangia found at the base of the strobilus in many species of *Lycopodium*, *Selaginella*, *Isoetes* and *Psilotum* to be vestigial.

(3) *Arrested or Atrophied Organs.*—These also persist in adult configuration and it is only the comparative or the developmental history that reveals their true character. In the adult leaflet of *Nephrolepis*, *Osmunda* and some other ferns there is a small auricle at the base of the leaflet. This little organ represents the third lobe of the embryonic pinna which is tri-foliate and acquires its elongated adult form by suppressing the basal lobes through a series of developmental changes. Similarly on the adaxial surface of the sporocarp of *Marsilia* there are two or more teeth present which represent arrested pinnæ and confirm the foliar nature of that organ. The common kidney-shaped indusium of *Nephrolepis*, *Nephrodium* and other genera is supposed to have been derived from the cup-shaped indusium split into two parts outer and inner as in some Davalloid ferns. The inner indusium does not develop. Only the outer one develops and forms the usual kidney-shaped type. But in some genera of the Davalloid ferns to which *Nephrolepis* belongs the inner indusium is seen in a rudimentary form, *e.g.*, in *Hypolepis* (see Bower,[27,28] 1923, pp. 221-223 and 1928, p. 11). A similar vestigial indusium is also found in *Marsilia* and *Pilularia* which suggests their affinity with the Schizæceæ.

The spore-producing parts of the Ophioglossaceæ were a very controversial topic since long; but the discovery of the early Devonian plants in which sterile and fertile parts have been associated together, called telome, threw a new light on these structures. At the distal ends of the little sporangiophores arranged in two rows in *Helmonthostachys* there are clusters of small leafy outgrowths. These little appendages were meaningless so far. But now with the help of our knowledge of the Devonian genera we interpret these as vestiges of leafy parts which were of commensurable dimensions in the ancestors of the Ophioglossaceæ. The affinity of the Ophioglossaceæ to the Cœnopteridinæ, though remote, is largely based on this fact.[29] The same is probably the interpretation of the ligule of lycopods, *Selaginella* and *Isoetes* and also of the buds sometimes noticeable in the seedlings of *Osmunda* and in *Botrychium*,[30]

(4) *Vestigial Tissues.*—The last category of the vestigial parts found in the vascular cryptogams are the internal organs reduced to a vestigial state, the so-called vestigial tissues. A large number of examples of these are known; but I do not propose to survey them all here. Only a few striking examples are cited below.

It is well known that the centripetal xylem is not seen in the stem of living *Equisetum* but it was quite a general condition of the whole stock in the anatomy of Protocalamites. Curiously enough this condition is seen in the traces of the reduced vegetative leaves of *E. maximum* and in the traces of the reproductive leaves of *E. palustre* and *E. hiemale*. The condition here is identical with that of the leaf-traces in Lycopodinæ.

Another good example is to be found in the leaf-trace of the living Cycadales and the Cycadofilicales. In the Cycadofilicales of the Palæozoic period the bundles of the stem were always characterized by the presence of centripetal or cryptogamic xylem. But this is conspicuously absent in the stem of the living Cycadales. However, there is a clear and universal presence of centripetal wood in the foliar fibro-vascular bundles of the living Cycadales.

Similar vestigial traces of xylem are also found in the teeth on the sporocarps of *Marsilia* and *Pilularia* and in the ochreola noticeable at the bases of branches of *Equisetum* species. In many species of *Equisetum* ochreola lack vascular traces; but Milde[31] (1867) has found them in the ochreola of *E. arvense*, *E. limosum* and *E. hiemale*. It is well known that the nodal structure of *E. variegatum* and allied species where remnants of siphonostele are noticeable has been interpreted to be vestigial by many competent authors. The cambium found in the stem of *Botrychium*, *E. maximum* and *Isoetes* is also of a similar nature.

All these examples are quite sufficient to show the widespread occurrence and the variety of structures called vestigial in the vascular cryptogams. In dealing with them, it is obvious that the concept when structures are being considered is qualitative; and it is quantitative when the functions are being considered. This distinction, however, is not well recognised in botanical literature; but to my mind it is of considerable theoretical importance as will be seen from what follows.

THE ORIGIN OF VESTIGIAL ORGANS: A RATIONALE

We shall now turn to some theoretical considerations. How can the origin of the vestigial organs be conceived? There is but little positive information to which we can turn in answer to this question. We have no doubt sufficient information at our disposal regarding the course of their development and degeneration also but that does not take us much further. It is possible, nay probable, that an organ having gone out of use for several generations thriving under the same set of environments for millions of years may have been atrophied and reduced to a state of a rudimentary structure in heredity. But how can disuse ever act on an organ never used and already reduced and reduce it still further to a state of vestige? The principle of economy of materials in an expanding form of the body may perhaps be one of the reasons for this: but that does not solve the whole problem as there are cases on record where no such economy seems to have been effected, *e.g.*, in the production of a small papilla consisting of a few cells in a flower. The difficulty is genuine and has been well recognized by many investigators. Here is an example:

> "After an organ has ceased being used, and has become in consequence much reduced, how can it be still further reduced in size until the merest vestige is left: and how can it be finally quite obliterated? It is scarcely possible that disuse can go on producing any further effect after the organ has been functionless. Some additional explanation is here requisite which I cannot give."[32]

These lines come from an ardent investigator no less than Darwin himself. But we must remember that these lines were written by him in 1858 when he had not had any occasion to see much of the later development in the science of modern Biology. Too great a faith in orthogenetic continuity of species as Darwin had, can lead to no other conclusions than these. But with the recent advancement of the surging wave of mutation theory, the breaks in the life lines now appear to us to be more real than apparent. By some fruitful chance, by some strange reorganization of the chromatin matter, by some sudden change in temperature either hot or cold, by an unknown chemical or bacterial stimulus, by the action of some unseen and unknown radiations, the nucleus of a cell in a species undergoes gene-mutations which result in bringing out new forms with new morphological characters, howsoever small they may be; and some of these do persist in times to come. Some of these characters are stable and useful and help the organism in adapting itself to its environments and in mastering them better; and others are equally useless for all that. A few others are of doubtful utility and get simplified and reduced to a rudimentary state in course of time. Some of these useless features get secondarily adapted to some purpose other than the original one for which they were meant and become partly vestigial and partly useful. Still others result in monstrosities and bring forth odd forms with strange characters. Many of these are quite unstable and are soon lost in heredity, but some do persist and become vestigial. How significant in this connection is T. H. Morgan's finding that quite a large number of mutations he was able to induce in *Drosophila* resulted in the atrophy of some parts and were both unstable and useless.[33] But some of the gene-mutations of doubtful use do persist in the body of an organism. May it not be that in the plants also some such mutations have given rise to changes which resulted in producing parts apparently useless, at any rate of doubtful utility. Natural selection having acted upon these and having found them useless may have reduced some of them to a rudimentary state and others to a state of vestige only. However, it is necessary to state here that I do not want to suggest that every vestigial organ owes its origin to gene-mutations, though I do feel that many of them may have arisen that way. Natural selection having acted upon

them and having found them to be of no direct use to the organism, may have reduced them to a vestigial condition. Truly vestigial organs, therefore, would be those that have arisen in heredity qualitatively as parts *de novo* on account of gene-mutations and have persisted in heredity notwithstanding the plain stamp of inutility they might have obtained later. Herein then perhaps lies the rationale of a problem left quite open by the unbiased mind of Sir Charles Darwin.

I take this opportunity to thank Professor J. J. Asana for his kindness in going through the manuscript and making me some useful suggestions.

1. Darwin, C., *The Origin of Species*, 1888, John Murray and Co. London 2. Hækel, Ernst, *Generelle Morphologie*, 1886, Berlin. 3. Darwin, *loc. cit.* ch. **14.** 4. Goebel, K., *Organography of Plants.*, 1900, Part 1. English Ed. 5. Bower, F. O., "Imperfect sporangia in certain pteridophytes. Are they vestigial?," *Ann. Bot.*, 1901, **15**, 225–267. 6. —, *Primitive Land Plants*, 1935. 7. Jeffrey, E. C., *The Anatomy of Woody Plants*, 1917. 8. Sahni, B., "On the theoretical significance of the so-called 'abnormalities' in the sporangiophores of the Psilotaceæ," *Journ. Ind. Bot. Soc.*, 1923, **3**, 185-191. 9. —, "The ontogeny of vascular plants and the theory of recapitulation," *Op. cit.*, 1925, **4**, 202–216. 10. Browne, I. M. P., "On a new theory of Calamarian cone," *Ann. Bot.*, 1927, **41**, 301–320. 11. Bower, F. O., *The Ferns*, 1923, Vol. **1**, Cambridge. 12. Campbell, D. H., *The Eusporangiatæ*, 1911. 13. Kidston and Lang, W. H., "On Old Redsandstone plants showing structure from the Rhynie Chert bed, Aberdeenshire, Parts I–V," *Trans. Roy. Soc.*, Edinburgh, 1917-1921, **51-52.** 14. Holloway, J. E., "The prothallus and young plants of *Tmesipteris*," *Trans. Proc. N. Z. Inst.*, 1917, **15**, 1-44. 15. Lawson, A. A., "The gametophyte generation of the Psilotaceæ," *Trans. Roy. Soc.*, Edinburgh, 1917, **52**, 93–113. 16. Lyon, H. L., "A new genus of Ophioglossaceæ" *Bot. Gaz.*, 1905, **4**, 455–458. 17. Duthie, A. V., "Studies in the Morphology of *Selaginella pumila*, Part III," *Trans. Roy. Soc. South Africa*, 1926, **11**, 275–295. 18. La Motte, C., "Morphology and Orientation of the embryo of *Isoetes*," *Ann. Bot.*, New Series, 1937, **1**, **4**, 695–711. 19. Treub, M., "Etudes sur les Lycopodiacees," *Ann. Jard Bot. Buitenzorg*, 1884–1888, **4-5** and **7**. 20. Goebel, K., *Organography of Plants*, 1904 Part II. 21. Bower, F. O., *The Origin of a Land-flora*, 1908. 22. Holloway, J. E., "Studies in the New Zealand species of the genus *Lycopodium*," *Trans. N. Z. Inst.*, 1917–1920, **48-52.** 23. Treub, M., *loc. cit.*, 1890, Part VIII, Considerations theoretique, pp. 23–37. 24. Holloway, J. E., *op. cit.*, Part IV, "The structure of the prothallus in five species," *Trans N. Z. Inst.*, 1920 **52.** 25. Darwin, C., *op. cit.*, 1888, pp. 397–402. 26. Johnson, A. A., "Hydathodes in the genus *Equisetum*," *Bot. Gaz.*, 1937, **99**, 598–608. 27, 28 Bower, *The Ferns*. 1923, Part I and Part III, 1928. 29. See Bower, *op. cit.*, 1935, pp. 361–365 and p. 394–395. 30. See Lang, W. H., "On some deviations from the normal morphology of the shoot in *Osmunda regalis*," *Mem. and Proc. Manchester Lit. and Phil. Soc.*, 1924, **68**, 53–67 and "Studies in the morphology and anatomy of the Ophioglossaceæ," *Ann. Bot.*, 1914, **27**, 203. 31. Milde, J., *Monographia Equisetorum*, *Nova Acta etc.*, 1867, **32.** 32. Darwin, *Origin of Species*, *op. cit.*, 1888, p. 401. 33. Morgan, T. H., *The Theory of the Gene*, 1932, Chs. 18 and 19 and Ch. 6, p. 75 and pp. 72–94.

Note.—The cost of printing this contribution has been defrayed by a generous grant from the Rockefeller Foundation for the publication of results of scientific work made to us through the kindness of the National Institute of Sciences, India.—*Ed.*

OBITUARY

ALEXANDER BOGOMOLETS

(1881-1946)

IN the death of ALEXANDER BOGOMOLETS the Soviet people have lost one of their best and most devoted scholars. He was born sixty-five years ago within the dreary walls of Kiev prison where his mother had been imprisoned for revolutionary activities by the Czarist regime. As a child he was marked for his exceptional abilities. After a brilliant high school career he joined the medical faculty in Odessa University in 1900, passed his final examination with honours in 1906, and was appointed Assistant Professor of Pathology. His researches on "Structure and functions of suprarenal glands both in the healthy and sick organisms" got him his doctorate in 1909. After one year's work in the physiological laboratory at Sorbonne he was appointed Professor of General Pathology in Saratov University which post he held till 1925 when he was elected Professor of Pathological Physiology at the Second Moscow University. On the death of Bogonov, Bogomolets succeeded him as the Head of the First Blood Transfusion Institute, Moscow.

Bogomolets' researches and contributions are varied and many. He established the lipoid nature of the secreta of cortex of suprarenal glands and originated the idea of iono-endocrinous regulation. He attached great importance to the reticulo-endothelial system and its role on longevity and immunity. He showed that the disturbances in the functions of this system led to a number of ailments and to premature old age. By causing immunity in animals using elements of reticulo-endothelial system he obtained a serum which was used in the U.S.S.R. with particular success during the war in the treatment of wounds and fractures. Bogomolets and his school undertook intensive study of conditions which facilitate longevity in certain parts of U.S.S.R. and elaborated modes for preventing premature old age and prolongation of life.

His publications include important works in the sphere of immunity, anaphylaxis, allergy, pathology of blood circulation and mechanism of the action of blood transfusion. In 1929 he was elected member of Academy of Sciences, Ukranian S.S.R., of which he subsequently became President. In 1932 he was elected member of the Academy of Sciences of the U.S.S.R. He achieved the highest honours possible in the Soviet Union; twice he was elected deputy to Supreme Soviet of U.S.S.R., and was the Deputy Chairman of Supreme Soviet of Ukranian S.S.R. He was one of the recipients of the "First-class Stalin Prize".

LETTERS TO THE EDITOR

A NOTE ON "THE POSSIBLE EFFECT OF THE ATOMIC BOMB TEST AT BIKINI ON RADIO RECEPTION", AT ABOUT 3·05 A.M. (I.S.T.) ON 25th JULY 1946

IT is known that an atomic bomb on bursting will give rise to a considerable mass of ionised vapour at a pressure of 10^8 to 10^9 atmospheres and a temperature of several million degrees. A large amount of radio-activity will be produced, part of which may decay quickly whereas some portion may persist. It is, therefore, expected that there may be ionization over the surface covering a very large distance and that the ionospheric conditions at lower or greater heights may also be affected. A study of the propagation of radio signals as well as atmospherics originating from points situated at large distances from Bangalore in the Bikini-Bangalore direction around the time of the test was undertaken.

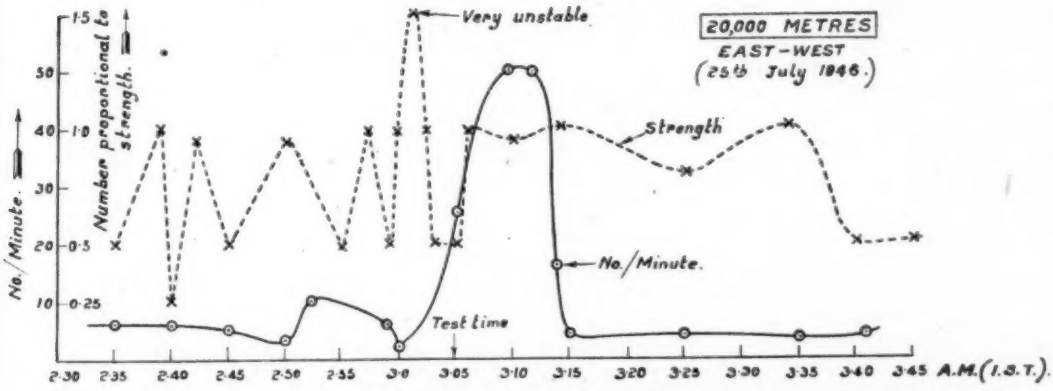
Bikini Lagoon is situated about 6,250 miles more or less due East of Bangalore (to be exact, 2°·5 South of due East of Bangalore). The experimental arrangements set up for *simultaneous operation* at the Department of Electrical Technology and the D.F. Hut of the Indian Institute of Science, Bangalore, India, were as follows:—

(1) British Radio Research Board Directional Recorder (for atmospherics) operating on 20,000 metres.—This was set up for study of the propagation of atmospherics on 20,000 metres from that direction (the origin of such atmospherics might be at very large distances from Bangalore and their transmission path would mostly be over the sea). The recording was arranged on the longest possible wavelength in long-wave band to include atmospherics originating at greatest distances.

(2) A sensitive field strength measuring arrangement set up for measurement of the electric field intensity of an American short-wave station on 25·3 metres in the direction of Bikini at Bangalore. Since a portion of the transmission path through ionosphere was in light and the other in darkness during the period of observation, the American station of 25·3 metres wave-length was selected for observation in preference to others on wave-lengths shorter or longer than 25·3 metres.

The research workers taking part in the observations were Messrs. B. N. Prakash, S. M. Das-Gupta, N. V. Narayana Rao, J. Das, Amal Ghosh and S. K. Chatterjee. The time occupied in the observations was from 12 midnight to 5 a.m. (I.S.T.) on the 25th July 1946 (the atomic bomb test came off at about 3.05 a.m. I.S.T.).

Analysis of the observations made with the Directional Recorder operating on 20,000 metres has shown that the number of atmospherics per minute increased suddenly to a high figure at 3.05 a.m. and remained at a high level till 3.14 a.m. and that the strength of the disturbance varied rapidly remaining very unstable till 3.05 a.m. and subsequently became quite stable. Fig. 1 shows the observations for the period 2.30 to 3.45 a.m.



Results of observation on the American station on 25·3 metres have shown the following :—(*a*) The electric field intensity at Bangalore decreased considerably at about 3.05 a.m. and remained at a very low value from 30 seconds to 1 minute with the result that Signal/Noise ratio became extremely poor for reception. (*b*) The electric field intensity at Bangalore became negligibly small (similar to that in a "radio-fade out"), and the noise level very high from 3.52 a.m. to 3.55 a.m. so that the station was completely masked for about 3 minutes. From 4 a.m. onwards, the reception of the same station was fairly good, as before 3 a.m.

No definite conclusions regarding ionization either at lower (below 'E' layer) or greater heights or over the surface can, however, be drawn from the results of the above experiments due to a few uncertain factors. The note is meant to be a record of the effects observed on the reception of radio signals and atmospherics between 2.30 and 4 a.m. on the 25th July 1946.

Dept. of Electrical Technology,
Indian Institute of Science,
Bangalore, S. P. CHAKRAVARTI.
August 3, 1946.

THE APPARENT ENLARGEMENT OF THE SUN AND THE MOON NEAR THE HORIZON

IT is a matter of common experience that the rising or setting sun and moon appear oversized when near the horizon. It is hard to believe, but true, that they are no bigger at the horizon than when they are at the zenith. The explanation for this phenomenon cannot be sought in atmospheric refraction; for, such refraction has the effect of causing not an enlargement, but indeed a slight diminution in the horizon sizes of the sun and the moon.[1] If we ignore this slight diminution, then there is hardly any change in the sizes of the images of these luminaries on the retina, as they move across the horizon. To explain this observation on purely geometrical grounds is thus impossible. The view that is generally held[2] seems to be that this illusion is an error of interpretation by the eye, which unconsciously adopts different scales for the measurement of objects in the sky according to their distances from the zenith. Due significance does not, however, seem to have been given in this connection to the sharp variation in the apparent luminosity of the sun and the moon when passing the horizon. The considerations set forth in this note would show that the tendency for the eye to employ diverse scales for the estimation of the sizes of the sun and the moon at the zenith and the horizon can be traced to the sharp variation in the apparent luminosity of those bodies as they move across the horizon.

For the sake of simplicity, we shall first consider the case of the setting sun. During the process of the apparent movement of the sun in the sky on any day, it is easily seen that the physical distance separating the earth from the sun is unchanged. The actual diameter of the sun is also an invariant quantity. The angle subtended at the eye by the sun is therefore constant, neglecting the slight detractive effect of atmospheric refraction. The apparent brightness of the sun in contrast with the background is, however, very much more at the zenith than at the horizon. For, as the sun nears the horizon, there is increasing absorption and scattering of the sun's light by the atmospheric constituents. As a result of this, the intensity of the light reaching the eye from the sun is reduced and the setting sun becomes increasingly red. The sun would thus seem to lose its brilliancy as it nears the horizon.

While investigating the apparent shape of the sky at sunset, the author has found that, with gathering gloom at sunset, the horizon appears gradually to drift into the distance.[3] In a recent paper on the apparent shape of the sky,[4] the author has put forward arguments for the view that the human eye tends to place darkening objects at increasing distances from the eye. The waning luminosity of the setting sun would thus incline the eye to feel the sun to be moving farther away.

The subjective estimation of the size of any object is based primarily on the angle subtended at the eye by the object and its apparent distance. If two spherical objects, both

equally bright but at different distances, subtend to the same angle at the eye, the eye has learnt to associate a greater size with the more disant object. Let us suppose that, while the angle subtended at the eye by the same object remains unchanged, its apparent distances increases. Then, the farther the object seems to recede, the bigger would be the size the eye would associate with the object. The sun subtends practically the same angle at the eye throughout its apparent journey across the sky but would seem to go farther away from the observer as its brightness decreases at sunset. This apparent increase in the distance of the sun from the observer, while the subtended angle remains virtually constant, would have the effect of causing the illusion of an enlargement in its size.

The setting sun appears largest when its luminosity reaches its minimal value. Further, on days when the sun's luminosity is greatly reduced, as by a hazy atmosphere, we feel the sun's apparent diameter to be correspondingly enhanced. The apparent enlargement of the setting sun may thus be an effect arising out of the variation of its apparent luminosity relative to the background illumination.

The over-estimation of the sizes of the rising sun or moon and the setting moon may be similarly explained as originating in the waxing or waning luminosity of those bodies. Again, the illusion of the full-moon looking larger than the sun though both these bodies subtend about the same angle on the average at the eye of an observer on the earth, may be attributed to the small luminosity of the moon compared with that the sun has at the same elevation above the horizon.

The author wishes to thank Dr. S. R. Savur, Ph.D. (Lond.), Regional Director, Regional Meteorological Centre, Madras, for his kind interest in the work.

Meteorological Office,
St. Thomas' Mount P.O.,
Madras, D. VENKATESWARA RAO.
June 5, 1946.

1. Sir John Herschel, *Outlines of Astronomy*, 1878, 35. 2. Sir William Bragg, *The Universe of Light*, 1933, 62. 3. Venkateswara Rao, D., *Curr. Sci.*, 1946, **15**, 40–41. 4. —. *Effect of Illumination on the Apparent Shape of the Sky* (under publication).

AN INSTANCE OF THE OCCURRENCE OF *MONILIA ALBICANS (CANDIDA ALBICANS)* IN DENTAL CARIES

IT is nearly a century since informations on the nonascusporogenous and filamentous yeasts called "*Monilias*" began to accumulate, but we cannot as yet say that our knowledge regarding them has gone beyond a mere beginning of a systematic study. These micro-organisms have been shown to be associated with a number of pathological conditions and other habitats. From 1853[1] onwards scores of papers have been published in connection with these organisms, but no attempt will be made here to review all the important literature. The year 1923, however, marks an important date, as it is in that year that Berkhout[2] suggested the generic name *Candida* for these micro-organisms till then loosely classified among monilias. For it is recognized by all medical mycologists that it is incorrect to use the term monilia as a generic term. Benham,[3] however, is of the opinion that the name established by usage in medical literature be retained. Whatever generic term may be kept, it does not in any way minimize the intrinsic difficulties encountered in their identification or lower their significance in medical mycology. The more recent publications of Stelling-Dekker,[4] Langeron and Talice,[5] Lodder,[6] Martin *et al.*,[7] Langeron and Guerra,[8] Martin and Jones[9] and Conant[10] have, however, dispelled a great deal of difficulties and led to a more accurate recognition of these micro-organisms.

The monilias are usually associated with thrush, but recently they have been suggested to have a role in dermatoses and pulmonary diseases. They have been also isolated from cases of carcinoma, tuberculosis and even from individuals with no definite or demonstrable pathologic lesions. Even though it is not the intention of the present authors to assign any ætiological role to *Monilia albicans* in the occurrence of dental caries, nevertheless, this species had been once isolated from a case of dental caries of a molar in its third degree of decay.

The presence of this organism within the pulp of the tooth is interesting from more than one point of view. Firstly, even though yeast and yeast-like organisms have been associated with buccal flora, there is not in evidence, so far as these authors are aware, a case where monilia have been found within the tooth or associated with dental decay. Secondly, this organism was isolated from carious lesion after the tooth had been subjected to a drastic chemical sterilization process not hitherto employed for animal tissues, but which has been employed with success for isolating bacteria from the root nodules.[11] This process was employed with great success by the authors in connection with their work on dental caries[12] and in one case a monilia was isolated.

It was with great deal of difficulty that the isolated organism could be identified as monilia. It presented difficulties because of its slight but successive changes in morphological and cultural characters. When it was first isolated, it microscopically appeared exactly like a yeast—round or oval yeast-like budding cells that are rather irregular. Colonies were moist and creamy. But gradually the structure and consistency of the colonies changed, and dry well-developed "tree-like" mycelium with chlamydosphores became the feature of the colonies. In the beginning a smear could be made easily from the colonies and stained with any of the stains such as Loeffler's methylene blue; but when the transformation was complete, it was not possible to make a smear and constantly the morphological features had to be studied by the split-disc method of Vernon[13] and examined after staining with lactophenol. Such changes had been observed by several workers.

A complete physiological examination conducted on the lines recommended by Martin

et al.[7] and later reported by Mackinnon *et al.*[14] which included tests for growth in alcohol, assimilation of nitrogen, gelatin liquefaction, carbohydrate reactions, growth in corn-meal agar, etc., clearly revealed that the isolated organism was none other than *Monilia albicans* (Candida albicans). Even though the Duke Hospital Report[7] indicates 47 out of a total of 124 monilia cultures to have had their origin in sputum, still, in view of the fact that *M. albicans* has not yet been reported to have been found in the interior of a decaying tooth, it is hoped that this note will be of interest to those interested in monilias.

Microbiology Dept.,
St. Xavier's College, J. V. BHAT.
Bombay, MEENAKSHI V. SHETTY.
August 1946.

1. Robin, C., *Histoire naturelle des vegetaux parasites qui crissent sur l' homme et sur les animaux vivant*, Paris, 1853. 2. Berkhout, M. C., *Rev. Appli. Mycol.*, 1923, **3**, 555. 3. Benham, R. W. J., *J. Inf. Dis.*, 1931, **55**, 12-25. 4. Stelling-Dekker, N. M., *Die Sporogenen Hefen. Amsterdam*, 1931. 5. Langeron, M., and Talice, R. V., *Ann. Parsit Humaine et Comparee*, 1932, **10**. 6. Lodder, J., *Die anaskosporagenen Hefen. Amsterdam*, 1934. 7. Martin, D. S., Jones, C. P., Yao, K. F., and Lee, L. E., *J. Bact.*, 1937, **34**, 99-129. 8. Langeron, M., and Guerra, P., *Ann. Parasit. Humaine et Comparee*, 1938, **16**. 9. Martin, D. S., and Jones, C. P., *J. Bact.*, 1940, **39**, 609-630. 10. Conant, N. F., *Mycopathologica*, 1940, **2**, 253-66. 11. Harrison, F. C., and Barlow, B., *Centr. Bakt. II*, 1907, **19**, 264 and 426. 12. Bhat, J. V., and Meenakshy Shetty, "A Contribution to the Studies in the Aetiology of Dental Caries and Oral Hygiene," *M.Sc. Thesis, Bombay University*, 1945. 13. Vernon, T. R., *Ann. Bot.*, 1931, **45**, 733. 14. Mackinnon, R. C., and Artagavasta-Allende, *J. Bact.*, 1945, **45**, 317.

MARCASITE IN TRAVANCORE LIGNITE

THE occurrence of marcasite has already been reported in the Geological Reports of Travancore.[1] But no work on the chemical aspects of Travancore marcasite seems to have been done. The purpose of the present work is to determine the percentage of marcasite in Travancore lignite and also to explore the possibility of the utilisation of its sulphur content.

In Travancore, marcasite occurs distributed in the lignite beds at Varkalai and also in certain localities in the north. It is present as tin white cylindrical pieces varying in diameter from ¼" to 1" nd also as small concretions and nodules inside lignite. Representative samples were collected along with the surrounding lignite from six different localities. Each was crushed and weighed after drying in the sun. The marcasite in it was then separated by gravity-washing, and again weighed after drying. The results of this experiment with the six samples gave an average of 5·6 per cent. by weight of marcasite to be present in lignite. The specific gravity and chemical composition of marcasite as known[2] were identical with those of the mineral under investigation. On treating the mineral and also authentic specimens of pyrites with hydrogen peroxide, sulphur separated only in case of the latter, thus establishing that the mineral was marcasite.[3,4] Known weights of the mineral were roasted, separately in a combustion tube, and the gaseous oxides of sulphur evolved were absorbed in alkali. It was then oxidised by bromine water, and the amount of sulphur was estimated as barium sulphate. It was seen that the whole of the sulphur could be made available as gaseous oxides.

Although marcasite obtained as bye-product in coal mining industry has been used as raw material in the production of sulphuric acid in Germany and America[5,6] it is not possible to pronounce any opinion on the matter about Travancore marcasite because the extent of lignite deposits at Varkalai is under investigation and so the amount of marcasite that could be obtained is uncertain.

Chemistry Department,
University College, K. VISWANATHAN NAYAR.
Trivandrum,
July 26, 1946.

1. Records of the Department of Geology of Travancore, 1921, **1**, 37. 2. *Text-Book of Mineralogh*. E. S. Dana. 3. Grill, E., *Periodica Mineral*, 1932, **3**, 84-6. 4. Namiens, G., *Atti. Soc. Nat. Math. Modena*, 1933, **64**, 12, 1. 5. *Bruno Waser Metallborse*, 1930, **20**, 61-2-117 8. 6. Domke, K., and Behrisch, C., *Braunkohle*, 1928, **23**, 1005, 9.

CHEMISTRY OF KURCHI SEEDS PART IV. ISOLATION OF GALACTOSE FROM THE PICRIC ACID HYDROLYSIS OF THE GLYCO-ALKALOID

IN a recent communication to this *Journal*, the isolation of a glyco-alkaloid from kurchi seeds and its hydrolysis with aqueous hydrochloric acid have been reported.[1] It seemed advisable to use picric acid as the hydrolysing agent as it would also precipitate one of the hydrolytic products, *viz.*, the base as an insoluble picrate. The filtrate from the picrate, after treatment with lead acetate to remove phenolic bodies, was expected to yield the sugar. This method proved quite successful and led to the isolation of galactose which was characterised by its melting point, specific rotation, formation of mucic acid on oxidation with nitric acid, and the preparation of its osazone.

3·6 G. of the crystalline glyco-alkaloid[1] dissolved in 15 c.c. water, was treated with a saturated aqueous solution of picric acid till there was no further precipitation. 0·5 G. of precipitated picrate gave on crystallisation from alcohol 0·1 g. of sparingly soluble conessine picrate, m.p. 220-21° C., and the alcohol-soluble picrate yielded on crystallisation from aqueous acetone (1:1) 0·1 g. of yellow needles, m.p. 113-16° C., the base from which is being investigated. The filtrate after removal of picrates was treated with 20 per cent. lead acetate solution till no further precipitation occurred and the filtrate from the lead precipitate was de-leaded by hydrogen sulphide. The filtrate was decolourised with norite and the

decolourised solution evaporated to a syrupy residue (1·7 g.). The syrup on repeated extraction with cold absolute alcohol yielded 0·1 g. of sugar in the form of a white powder, m.p. 130-160°. 0·047 g. in 10 c.c. aqueous solution gave in a decimeter tube a rotation of $+0·40°$. Therefore $[\alpha]_D^{25} = +85°$. On oxidation with 1:2 nitric acid the sugar gave an alcohol-insoluble product, m.p. (190°-) 210° corresponding to mucic acid and with phenylhydrazine the sugar gave an osazone, m.p. 200° proving the sugar to be galactose.

My thanks are due to Mr. P. Ramaswami Ayyar for guiding the work and to Dr. P. C. Guha for kind interest.

Dept. of Pure & Applied Chemistry,
Indian Institute of Science,
Bangalore, (Miss) R. J. Irani.
August 5, 1946.

1. *Curr. Sci.*, 1946, **15**, 106.

REFRACTIVE INDEX OF MILK

In the course of experiments on the determination of refractive index of milk with the Abbè refractometer, the separation of fat from the milk enabled an accurate determination of the R.I. without affecting the value obtained. This figure, it will be noticed, represents, unlike the usual figures for milk sera, the true refractive index of *milk*. After a few trials, the following method was devised for the quick determination of the R.I. About 10 c.c. of milk in a Gerber butyrometer is centrifuged for five minutes in an ordinary milk centrifuge, the defatted milk carefully collected and tested for R.I.

Nearly 100 genuine samples each of cow and buffalo milk from the Military Dairy Farm, Hebbal, Bangalore, and about 20 random samples from animals under widely differing conditions of management in the City were analysed and tested for R.I. (40° C.) as described above. The R.I. of all the samples of cow milk was confined to the range 1·3449 to 1·3471, while that of buffalo milk lay between 1·3460 and 1·3492, the most frequent value for cow milk being 1·3450 and for buffalo milk, 1·3480. The refractive constant 'K' (Lorenz and Lorentz[1]) for each type of milk was more distinct, and fell within still narrower limits. For cow milk 'K' was between 0·2065 and 0·2075, and for buffalo milk, 0·2076 and 0·2088.

From the available data it is concluded that cow milk with R.I. $< 1·3449$ and $K < 0·2065$ and buffalo milk with R.I. $< 1·3460$ and $K < 0·2076$ can be considered to be adulterated with water. Viewing the R.I. and K in conjunction with each other, it is in general, possible to detect samples of watered buffalo milk designed to pass off as cow milk. In such instances, while the R.I. is too high for cow milk, K will be found too low for buffalo milk.

Added skimmed milk in buffalo milk can be detected, as a rule, upto about a minimum of 25 per cent. adulteration when the rising density tends to lower K without affecting the R.I. This is not, however, equally helpful with cow milk where larger additions of skimmed milk are possible without abnormally affecting the constants.

Added sugar in milk, watered upto a minimum of 10 to 15 per cent., can be detected by the lowering of both the R.I. and the refractive constant below the normal levels.

The details of the experiments will be published elsewhere.

My thanks are due to Mr. B. N. Banerjee and Prof. V. Subrahmanyan for their kind interest in these investigations.

Dept. of Biochemistry,
Indian Institute of Science, K. S. Rangappa.
Bangalore,
July 18, 1946.

1. Lorenz and Lorentz, *Food Analysis*, Woodman, A. G., Mc-Graw Hill Book Co., London and New York, 1941, 140.

INCOMPATABILITY OF FILTERABLE YEASTS

Sulc[1] in 1910 and Buchner[2] in 1912 have created new genera for some microorganisms which have been reported to be exclusively found in symbiosis with insects and have been named Cicadomyces, Aleurodomyces and Coccidomyces. So far no bacteriologist has come across with them and I have pointed this fact to throw one more doubt on their being considered living entities. However, there has been a single exception which is also to be interpreted as confirming my criticism. P. A. Lewis,[3] under the title of a filterable yeast-like microorganism, discusses microorganisms, found by Sulc in symbiosis with some insects, which, according to Lewis, were "classified by Sulc as the smallest of the yeasts; length 1-2 μ".

Lewis was not the only worker to have been misled by the special literature on symbiosis. In a previous communication I[4] have shown how the symbiote of *Tachardina lobata* was first considered a bacterium by me but on consulting the literature I subsequently looked upon it as an yeast. Further work, however, convinced me that my original view was the correct one. Lewis was an expert microbiologist; no one works on yellow fever virus as he did without perfect confidence in bacteriological technique. A mistake from such a worker requires an explanation. But let us consider facts first.

Lewis subsequently died of yellow fever as a martyr to scientific research and his successor, Dr. Schope, kindly sent me a reprint of his paper with other relative information. A culture of the filterable yeast, *Schizosaccharomyces filtrans*, was deposited by Lewis with the American Type Culture Collection, Chicago, which kindly supplied me with a tube. I was working then in the Institute of the late Prof. Breindl of Prague who was engaged on the virus of typhus fever but subsequently died of it. The filterable yeast proved to be a bacterium most allied to *Micrococcus roseus*, a culture of which was available

in Breindl's Institute. The organism of Lewis, however, produced a paler rose colour than *M. roseus*, whereas microscopically there was hardly any difference between the two. How Lewis could have mistaken a Micrococcus for an yeast can only be explained by the fact that statements in print are sometimes taken for unchallengeable facts. That bacterial cells can assume giant forms and occasionally resemble yeasts have been confirmed by many workers and Löhnis and others have built complicated theories of life-cycles among bacteria upon such findings.

It is only pertinent to mention that in another communication, I propose showing how Cicadomyces and allied genera are really tissue debris while most symbiotes of insects are bacteria proper.

Osmania Medical College,
Hyderabad (Dn.), S. MAHDIHASSAN.
July 23, 1946.

1. *Sitzb. K. Bohm. Ges. d. Wissenschaften*, Prag. 1910. 2. *Archivf. Prot.*, 1912, **26**. 3. *J. Exp. Med.*, 1927, **14**, 277-90. 4. *Curr. Sci.*, May 1946, **15**, 135.

THE CYTOLOGY OF YEAST

UNCERTAINTY still exists as to the exact number of chromosomes in *Saccharomyces cerevisiæ*, as found by various workers. It may be true as suggested by M. K. Subramanian[1] that "different chromosome numbers given by various authors may be due to studies of different races passing under the name of *S. cerevisiæ*". This, however, can also be attributed to not having used the best suited stain and fixative.

The technique employed in our study is the same as reported in our previous communication,[2] except for the fixative. The best fixative was Bouin's solution in which the smears were kept for forty minutes. Special treatment is, however, necessary to remove the last traces for picric acid for good staining. This is done by dipping the slides in 70 per cent. alcohol made alkaline (pH about 10).

In an actively dividing cell 12 chromosomes are seen which number is in complete agreement with Srinath's finding.[4] In the initial stages of division the chromosomes are seen in the middle of the cell, in scattered condition, as shown in Fig. 1. In the final stages the chromosomes arrange themselves in a very characteristic manner, forming a circle, as shown in Fig. 2.

There is some discrepancy between the findings of Srinath regarding centrioles. He mentions in one publication,[3] that one to five bodies are stained with Feulgen's reagent, but only illustrates four of them. He has further stated that they appear in the intranuclear vacuole, and regards them as nuclear material. In his next publication,[4] with a modified technique, only two bodies were stained with the Feulgen's reagent and he regards them as centrioles. This statement evidently shows that according to him one to five bodies which were mentioned as nuclear material are to be regarded now as centrioles. We observed that when *S. cerevisiæ* is stained with Feulgen's reagent certain bodies are stained which are not definite in size, shape and number. As the results are inconsistent it is not possible to arrive for the present at a definite conclusion.

FIG. 1.

FIG. 2.

FIG. 3.

X 1200

On the other hand slides stained with toluidine blue indicate the presence of two more bodies in addition to the chromosomes. They are constantly present in a dividing cell, and can be easily distinguished from the chromosomes by their small size and separate position in the cell, as shown in Figs. 2 and 3. Due to difference in structure, behaviour and the location of these bodies, they should be regarded as centrioles. We have already made a mention of these bodies in our previous communication.[2] Further work is under progress.

We desire heartily to acknowledge our indebtedness to Professor S. Mahdihassan.

Biochemistry Department,
Osmania Medical College,
Hyderabad (Dn.), MOHAN BABU NAIDU.
July 23, 1946. V. M. BAKSHI.

1. Subramanian, M. K., and Ranganathan, B., *Curr. Sci.*, May 1945, **15**, 132. 2. Mohanbabu Naidu and Bakshi, V. M., *Ibid.*, June 1946, **15**, 164. 3. Srinath, K. V., *Ibid.*, Jan. 1946, **15**, 25. 4. —, *Ibid.*, Feb. 1946, **15**, 50.

NUTRITIVE VALUE OF SOYA-BEAN AND RELATED PRODUCTS

WITH reference to your review[1] of the Report of the Soya-bean Sub-Committee of the Indian Research Fund Association, the following account of our recent work would be of some interest.

Practically all the work done by the Soya-bean Sub-Committee related to the use of the whole bean in the steamed or otherwise cooked condition as a *dhal*. In this direction soya-bean has proved disappointing and the conclusions reached by the Sub-Committee have been confirmed both by us at Bangalore and by investigators in other parts of the world.

In countries where soya-bean is finding very large application as an article of human food, it is mostly used either as a milk or as a

sauce. The former is a natural emulsion incorporating the protein, fat and minerals of the bean, while the latter is a pre-digested product. The work of the Soya-bean Sub-Committee had not included these two products.

The authors of the present note were present at the last meeting of the Soya-bean Sub-Committee which met in Delhi towards the end of November 1946. This meeting discussed the draft report of the Sub-Committee which was then being got ready for publication. It was the unanimous feeling of the Committee that a great deal more work on different aspects was needed, but that as the Committee had already completed a certain programme of work, the available material should be published. The draft report had gone beyond the actual work done by the Committee and as already fresh evidence was coming forth to reveal the higher nutritive value of soya-bean in a processed form, the report was modified in a number of places to provide scope for fresh developments. In fact, even at this meeting, both we from Bangalore and Dr. K. P. Basu from Dacca adduced evidence to show that soya-milk had a supplementary value when added to a rice diet, whereas the whole soya-bean had none.

At their meeting during the Autumn of 1944, the Soya-bean Sub-Committee had decided to close their work. Reference to this may be found in their earlier report. With this as a background, we started our work, about twenty months ago, studying the effect of each step in processing on the nutritive value of the resulting product. Independent evidence was also accumulating, chiefly in America, to show that the biological value of the protein was not a constant entity, but depended on the method of processing employed. By incorporating a number of improvements such as incipient germination, extraction to remove colouring matter and bitter principle, fine mechanical pasting, adjustment of reaction, and boiling under certain standard conditions, we showed that it is possible to obtain a vegetable milk which has the same properties as animal milk, at a fraction of the cost of the latter. We showed that the protein of the milk has a higher digestibility than that in cow's milk; that the biological value is not much lower and that the net values of the two proteins are practically the same; that the vitamin B complex of the two milks are of the same order; that, when added to the poor rice diet, soya-milk has a supplementary value corresponding to about 80 per cent. of that of the best cow's milk; that extended germination to about three days yields a protein with a higher biological value than that in cow's milk; that supplementing with calcium leads to further increase in nutritive value. We have also studied the effect of combining soya-bean with the commoner pulses, legumes and cereals with a view to producing a still better milk, but that part of the work is not relevant to the present subject.

We did not merely stop with the laboratory work and animal feeding experiments. We conducted an extended series of consumer trials with the milk, curd and related products. Thousands of people have sampled our products. The various products and, particularly the sour curd, have been much appreciated by all the users. Food preparations incorporating soya-milk or curd are indistinguishable from those prepared out of cow's milk.

With the above as a background and with the collaboration of the Health authorities of the C. & M. Station, we have been conducting a series of feeding experiments with the children in the local Welfare Centres. Soya-milk is being compared with cow's milk for feeding children ranging in age from a few months to seven years. The study is not yet complete, but the trends show that, especially in very young children, soya-milk produces better response than cow's milk. Experiments have also been recently started providing soya-curd and rice as a mid-day meal to well over a thousand primary school children. The number of children would have been much larger but for the fact that we are not at present in a position to supply more than about 400 lbs. of curd per day. In this connection, it may be mentioned that the panel of selection (which included the Rationing Adviser to the Government of India) actually preferred the rice prepared with soya-curd to that with cow's milk curd. Experiments will also be soon started comparing soya-milk with cow's milk in children's hospitals. Preliminary trials have already shown that children and invalids digest soya-milk more easily than cow's milk and that there is absolutely no ill-effect resulting from the use of the latter.

Side by side with the above, the technological side relating to the large-scale production of milk is being developed. Even with the limited equipment at our disposal, we could now produce over 1,000 lbs. of milk per day. Our present production is about 550 lbs., but we hope to double it at a very early date.

Thanks to the generous support of the Council of Scientific and Industrial Research, the Food Department, the Lady Tata Trustees and the C. & M. Station, Bangalore, we have already got a fairly big team of research workers on the subject. Further support will soon be forthcoming. Every aspect of the subject will be studied not only with a view to providing a complete scientific background but also to standardise the conditions for preparing a completely balanced vegetable milk that will have a higher nutritive value than the best grade dairy-fed cow's milk. We have already obtained promising results in this direction.

During the past twelve months, we have published some technical and popular articles bearing on the milk problem in the country;[2] the importance of processing in determining the nutritive value of soya-bean;[3] preparation of soya-bean milk.[4] Our technical papers relating to the preparation of soya-sauce,[5] biological value of soya-milk protein,[6] vitamin B complex of soya-milk,[7] supplementary value of soya-milk to rice diet,[8] and *in vitro* digestibility of soya-milk[9] are under publication. Further work bearing on the effect of combining soya with groundnut as also certain cereals and pulses, and on the calcium fortification of soya-milk

has been completed and is now being written up for publication.

S. S. De.
V. Subrahmanyan.

Dept. of Biochemistry,
Indian Institute of Science,
Bangalore,
July 8, 1946.

1. *Curr. Sci.*, 1946, **15**, 158. 2. *Sci. and Cult.*, 1946, **11**, 692. 3. *Ibid.*, 1945–46, **11**, 437. 4. *Curr. Sci.*, 1945, **14**, 204; *Ind. Farming*, 1946, **7**, 17; *Bull.* 7 of *the Food Conservation League, C. & M. Station, Bangalore*, 1946. 5. *Ind. Farming*, 1946, under publication. 6–9. *Annal. Biochem. and Expt. Med.*, 1946, accapted for publication.

ACCLIMATISATION OF *CYPRINUS CARPIO* TO THE PLAINS WITH NOTES ON ITS DEVELOPMENT

Besides the English carp* *Cyprinus carpio* proper, three varieties of the species, *viz.*, the Mirror carp, the Scale carp and the Leather carp, are found to thrive in the Ootacamund waters. A brief account of the introduction of the Mirror carp in the Nilgiris was recently given by Chacko (1945).[1] The English carp was acclimatised to the lower elevations as at the Sunkesula fish farm (1,000 ft.) in the year 1923. Recently we have succeeded in bringing down the Mirror carp also to the plains straight as far as the coast of Madras. In October 1945, five fingerlings of Mirror carp, of an average length of 5·5 inches were successfully transported direct to Madras and introduced in a pond in the Chetput Farm.

Without any previous conditioning, six fingerlings ranging from 5·3 inches to 5·7 inches were taken in a double tin carrier at mid-day on 28-10-1945 from Ootacamund. At Coimbatore the water was partly renewed (about half) at 7 p.m. and a twenty-pound block of ice was placed over the perforated lid of the tin carrier. The gradually melting ice, dripping down, kept the water in the tin fairly cool. On reaching Erode, at 10 p.m. the water was again half renewed. The block of ice kept on cooling the water till about an hour before reaching Madras and all the fingerlings arrived at the destination next morning in good condition. The tin was brought and kept partly immersed in the Chetput pond for a couple of hours after which the fish were transferred to a conditioning box in the same pond. By noon three fingerlings were found showing signs of distress, swimming at the surface often upside down in a giddy manner. By the next morning one specimen was found dead while the rest moved about in a perfectly normal condition. Feeding was tried at 10 a.m. dropping crumbs of bread and the fish were seen to nibble at them. Thus the fish stood the transport of 355 miles, from an altitude of 7,000 ft. practically to the sea-level and adjusted themselves to the warmer environments in a remarkably short time. The temperature of water at Ootacamund was 17·2° C. and that at the Chetput pond 30·64° C. The fish thrived well in the pond at Chetput which also contained Gourami, Catla, Pearl-spot, Mullets and Murrels. On 24th December 1945, a specimen which was netted measured 9·8 inches in length representing a growth of 4·3 inches in 55 days. Two specimens were again netted on 11th March 1946 and they measured 15·0 and 13·5 inches in length and weighed 30 and 28 ounces respectively. They were netted for a third time on 5th May 1946 when they measured 17·5 (weight 2 lb. 14 oz.) and 15·6 inches (weight 2 lb. 9 oz.) respectively. The larger specimen was a mature male. The rate of growth is remarkably quick—about 2 inches per month—and it clearly indicates the adaptability of the species to different environments involving wide ranges of temperature and other hydrobiological factors.

Further consignments of fingerlings have recently been successfully transported to Madras and stocked in the farm ponds, and additional data on their growth and maturity in the new environments are being gathered.

Breeding is found to be at the maximum from January to March in the Nilgiri waters. With a view to properly identify the larvæ and fry of the different varieties, attempts were made to strip and artificially fertilize the eva. Several oozing male and female specimens of English carp were obtained in March from the Ootacamund lake and on 16-3-1946 a batch of several hundreds of ripe eggs were stripped from an oozing female and successfully fertilised by milt obtained from an oozing male. The fertilized eggs were carefully reared to hatching. The fry are thriving well in aquaria on artificial feeding. The different stages in development have been carefully followed and a detailed account of the same together with interesting features of bionomics of the species are under preparation for publication.

Our thanks are due to Dr. T. J. Job for his kind suggestions.

K. H. Alikunhi.
V. Ranganathan.

Freshwater Biological Research
Station, Govt. Fisheries,
Madras,
May 18, 1946.

* The English carp differs from the varieties of *C. carpio* in several features and the details of the systematics of the species are under examination.

1. Chacko, P. I., *Jour. Bombay Nat. Hist. Soc.*, 1945, **45**, No. 2.

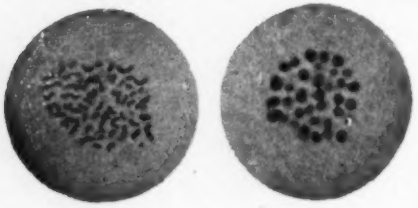
Published with the kind permission of the Director of Industries and Commerce, Madras.

CHROMOSOME NUMBERS IN BAMBUSEAE

In a previous communication[1] it was mentioned that the number of chromosomes in *Bambusa arundinacea* is $n = 35$ and $2n = 70$ though the previous record[2] for the same was $2n = 72$. I have had occasion in 1942 to examine the root and flower material of *Dendrocalamus strictus*, the flowering stalks of which were grown at the Sugarcane Breeding Station, Coimbatore, for hybridisation work. From a very careful examination of the chromosomes both at mitosis and meiosis it is found that

this species has also the chromosome number $n = 35$ and $2n = 70$, while Riccharia[3] and Janaki Ammal[4] record $2n = 72$ from root tip counts. Almost all the chromosome numbers recorded for the bamboos are from root tip counts as these flowers very rarely and it is really fortunate that flower materials could be obtained for the species mentioned here.

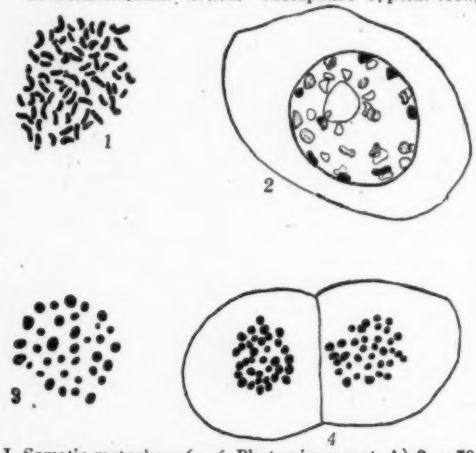
The chromosomes are figured at diakinesis, metaphase I and II of meiosis and also photomicrographs of somatic and meiotic metaphases are put in to show the nature of the preparations wherefrom this number is recorded.



A B

A. *Dendrocalamus strictus*—Somatic metaphase, polar view.

B. *Dendrocalamus strictus*—Metaphase I, polar view.



1 2

3 4

1. Somatic metaphase (*c. f.* Photomicrograph A) $2n = 70$
2. Diakinesis
3. Metaphase I. (*c.f.* Photomicrograph B) $n = 35$.
4. Metaphase II.

From the published chromosome numbers[4] for the different genera of the bamboo tribe, it is found that mostly all of them come under either $n = 35$ and $2n = 70$, while Riccharia groups with the basic number $n = 12$. It is felt, however, that more intensive investigations in this tribe are necessary in view of the discrepancies in the chromosome counts of the above two species and also it can be seen that any likely error of one or two chromosomes in the counts may change our conception of the basic number of the tribe from $n = 12$ to $n = 7$ or *vice versa*.

Sugarcane Station,
Coimbatore, N. PARTHASARATHY.
June 25, 1946.

1. Venkatraman, T. S., and Parthasarathy, N., *Curr. Sci.*, 1942. **11**, 194–5. 2. Janaki Ammal, E. K., *Nature*, 1938, **141**, 925; Ucchikawa, I. *Imp. Bur. Pl. Genetics, Plant Breeding abst.*, 1936, **6**, 289. 3. Riccharia, R. H., and Kotwal, J. P., *Ind. Journ. Agric. Sci.*, 1940, **10**, 1033. 4. Darlington C. D., and Janaki Ammal, E. K., *Chromosome Atlas of Cultivated Plants*, 1945, George Allen & Unwin Ltd., London.

ANOTHER PROBABLE ORIGIN OF THE WORD CHEMISTRY FROM THE CHINESE

IT has been previously[1] suggested that the word Chemistry is a Chinese derivative and that the French word, Chemie, sounds very much like the original. The Cantonese term Kim-Mi, signifies gone astray in the search for gold. Names are usually given by others. For example, Protestants, who now designate themselves as such, were so called, by people unsympathetic to them. Similarly Kim-Mi, or misplaced enthusiasm for gold, would be a name given by the critics of alchemy. Moreover, there has always been a greater preponderance of critics than of enthusiasts of alchemy so that the term which acquired currency, must have been the one used by the majority. It was such a consideration that formed the basis of my earlier[1] communication.

There is only one other possibility; to discover the name used by the alchemists themselves for the knowledge they sought. Such a name could have only signified the *secret of gold making*. In support of such a supposition I may quote from W. A. P. Martin's book, *Hanlin Papers*, 1880, p. 227, as follows: "Some find it in the mythology of the Greeks, maintaining—an interpretation older than the Christian era—that the golden fleece sought for by the Argonants, was merely a sheepskin on which was inscribed the secret of gold making. This construction of the legend comes from Dionysiun of Mitylene, who lived *circa* B.C. 50." The Chinese language, however, requires brevity and the condensed expression for the *Secret of Gold making* would be Gold-Secret, a term which, in classical Chinese, would again sound Chin-Mi, identical with the previous one meaning madness for gold.

The word secret, in Chinese, is Mi, as given in Mac Gillivray's *Dictionary of Chinese*, 1922, p. 599, and also in C. H. Fenn's *Pocket Dictionary*, 1932, p. 298. Unfortunately the word is also pronounced Pi and it is this sound which is mentioned in the *Chinese Dictionary* by Giles, 1892, character No. 8932. Pi is also its Cantonese pronunciation; so the term meaning Gold-Secret, in this dialect, would sound Kim-Pi which is phonetically different from Chemie.

The standard pronunciation Chin-Mi, meaning Gold-Secret, is also the one current in Ssch'uan. The Greeks would pronounce these words as Kin-Mi which, when freely spoken, would be easily converted into Kim-Mi and can serve as the ultimate origin of the word Chemie. This explanation would also apply to the other term Chin-Mi, meaning madness for gold. I do, however, believe that the term which was introduced into ancient Greece was

the one that reached there through the sea route for Canton represented a higher degree of culture than any inland centre up north.

I may further quote from Martin's *Hanlin Papers*, p. 230: "The Rev. Dr. Edkins in a paper on Taoism, published about twenty years ago, was the first, I believe, to suggest a Chinese origin for the Alchemy of Europe." With such a historical background the word Chemie acquires a connotative sense which is lost if it is not traced to the Chinese.

SUMMARY

Chemie can have two probable roots—Kim-Mi, Madness for Gold, and Kim-Mi, Secret of Gold.

Osmania Medical College,
Hyderabad (Dn.), S. MAHDIHASSAN.
August 6, 1946.

1. *Curr. Sci.*, May 1946, **15**.

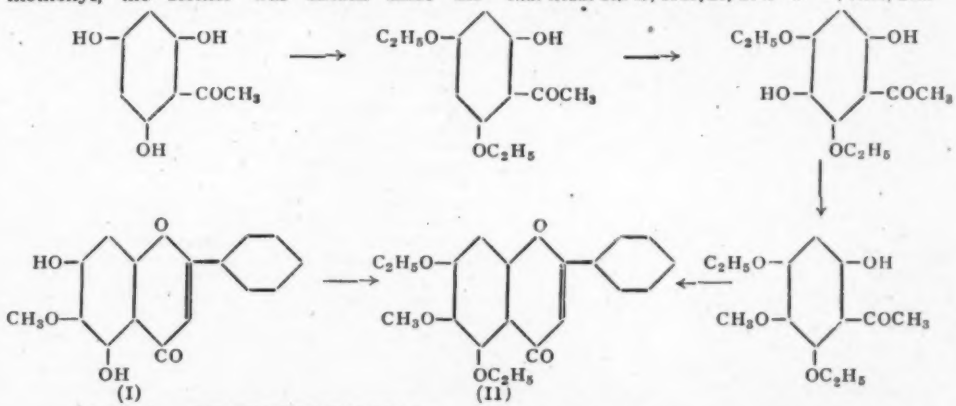
CONSTITUTION OF OROXYLIN-A

OROXYLIN-A was first isolated by Shah, Mehta and Wheeler[1] from the root bark of *Oroxylum indicum* and found to be a monomethyl ether of baicalein. In regard to the position of the methoxyl group the following points were taken into consideration. A hydroxyl group was resistent to methylation with diazomethane and this was placed in the 5-position. Of the other two alternative positions (6 and 7) for the methoxyl, the former was chosen since the substance did not give tests for the presence of ortho-dihydroxy grouping. Attempts to confirm this constitution by synthesis were unsuccessful.[2]

The preparation of the 7-methyl ether of baicalein has been recently reported by us;[3] its properties are quite different from those of oroxylin-A. Thus by elimination oroxylin-A should be the 6-methyl ether (I). Though the 6-methyl ether of baicalein could not be synthesised, it has now been possible to prepare synthetically a significant derivative of oroxylin-A providing positive proof regarding its constitution. O-Diethyl-oroxylin-A (m.p. 115-16°) is obtained by the ethylation of oroxylin-A using ethyl iodide and anhydrous potassium carbonate in anhydrous acetone medium. This is found to be identical (mixed m.p. undepressed) with 6-methoxy-5:7-diethoxy flavone which could be prepared by the new method of synthesis of 5:6:7-hydroxy-flavones and their derivatives recently described by Sastri and Seshadri.[4] The steps in the synthesis are indicated below:

Dept. of Chemistry, V. D. N. SASTRI.
Andhra University, T. R. SESHADRI.
Waltair,
July 30, 1946.

1. Shah, Mehta and Wheeler, *J.C.S.*, 1936, 591. 2. —, *Ibid.*, 1938, 1555. 3. Sastri and Seshadri, *Proc. Ind. Acad. Sci. A*, 1946, **23**, 273. 4. —, *Ibid.*, 262.



RACIAL CHARACTERISTICS

THE world is inhabited by a diversity of races. The origin of differentiation of mankind into such divisions is not known. Can climatic factors account at least partially for these differences? Experiments on isolated unstriated muscle support such a view (Singh, 1946). The optimum temperature for dog stomach varies with room temperature (Narayana and Singh, 1944). Similarly adaptation has been found in the tissues of the domestic fowl. Thus the optimum temperature for the œsophagus, which is more exposed, is lower than that for the duodenum; it is lower in winter than in summer. The optimum temperature for the testes in the human being would thus be lower than the tissues in the interior. The melting point of subcutaneous fat depends upon its distance from the exterior; this is to be correlated with the increase of temperature as the distance from the skin increases.

Such an adaptation would mean a redistribution of some factors in the tissues, for example ionic. This redistribution is bound to alter the characters of the individual. Similarly adaptation to diet, radiation is likely. Psychological characters may also play a part if nervous activity is accompanied by liberation of chemical substances or otherwise. There are thus bound to be differences between the English, the French, the Japanese, the Bengalees, the Punjabis, etc. Europeans dwelling in China may develop yellow characteristics if they live there as the Chinese do.

Dept. of Physiology, INDERJIT SINGH.
Dow Medical College, MRS. INDERJIT SINGH.
Karachi, M. C. MUTHANA.
May 25, 1946.

1. Singh, I., *Proc. Ind. Acad. Sci.*, 1946, **23**, 58.
2. Narayana, B., and Singh, I., *Ibid.*, 1944, **20**, 192

REVIEWS

Luther Burbank—A Victim of Hero Worship. By Walter L. Howard. (Waltham, Mass., U.S.A.: Messrs. Chronica Botanica Co.; Calcutta, India: Macmillan & Co., Ltd.), 1945-1946. Price 3.75 dollars.

This biographical sketch of the well-known "master-gardner, horticulturist and plant-breeder" Luther Burbank is authoritatively written by Dr. Walter Howard, Emeritus Professor of Pomology, University of California. Burbank's contribution to the building up of the reputation of California as the foremost Horticultural centre of the world has not been small in any manner of analysis. Nevertheless, the controversies that usually centre round great personalities have not been absent in the case of Burbank. Many men of pure science, Professor Howard says, had looked down upon the new varieties of fruit trees and ornamental plants that Burbank produced and even went so far as to say that they were merely imported from far off continents only to be renamed by the master showman Burbank. For instance, the Carnegie Institution paid a handsome annual grant of $ 10,000 for a period of five years and tried to get the scientific data and the entire story of the new hybrids that Burbank had claimed to have produced. For reasons not fully made known by the Carnegie Institution, their grant was suddenly withdrawn either because the scientific staff employed by them to act as *liaison* between Burbank and the progress of the science of Horticulture did not fully appreciate his work or because Burbank was not ready to divulge all his secrets. Whatever the reasons, a dispassionate and unbiassed judgement of the work done by this Horticulturist has now been written by Prof. Howard in this exceedingly fine publication. The reviewer was particularly attracted by the very genuine feelings of appreciation for Burbank's contribution to breeding better varieties of Horticultural plants expressed by the famous Dutch botanist, Professor Hugo de Vries. Other high lights of the book are Prof. Howard's narrative of the controversy of why Burbank's name was included in the list of scientists by the United States Post Office Department in 1940 when they decided to issue a three-cent commemoration BURBANK stamp. Among the illustrations, the reviewer was attracted by a charming photograph of the three practical men of America who were contemporaries in this century—Thomas Edison, Luther Burbank and Henry Ford. This picture was taken when Edison and Ford visited Burbank during the San Francisco Exposition in 1915 when Burbank was at the height of his fame.

The reviewer recommends this well-written book to all lovers of Horticultural development in this country. It brings out in excellent relief the one great quality of a practical plant breeder who with meagre fundamental scientific training, still felt it so necessary to give his best for the Nation's benefit. Particularly now, when the progress of Horticulture is in the threshold of great strides, honest practical men like Burbank would be a National acquisition.

T. S. SADASIVAN.

Geology for Engineers. S. Raja Raman, B.E., A.M.I.E. (Ind.). (College of Engineering, Trivandrum), 1946. Pp. 109. Price Rs. 4-8.

Professor Raja Raman being a Civil Engineer has taken considerable pains to bring out this small book on *Geology for Engineers*. The Engineers ought to feel grateful to the author for his attempt to present them with a useful book.

The book is divided into eight chapters: the first five deal with the General Principles of Geology, the subsequent two with portions connected with Engineering Geology, and the last with Economic Geology. While going through the text one finds lots of discrepancies which are bound to occur in a book written by one who has learnt the subject by himself. The author says that all that he has written are only things stated in other books. It would have added to the usefulness of the book if he had indicated the books in a Bibliography or as references. It is also felt that it would have been better if the author had taken the assistance of a Geologist in writing the book, rather than consulting the books himself. The discrepancies that I have mentioned are much too numerous for me to point them out individually. To speak of one or two, the author says that Microcline (p. 2) belongs to orthorhombic system. In p. 3, he gets confused between the habit of minerals and their crystalline forms. In p. 7, he places lava and the rock granite in the same category as rocks. In p. 19, he calls Magma as molten glass. In p. 31, while describing Mica-Schist the author makes it a character of this rock to contain fossil shells and corals. We find that the author is too ambitious to catalogue all the names in Geology without explaining them in their proper places. From the point of view of Civil Engineering Geology, it would be useful to refer here to Dr. Fox's book on *Civil Engineering Geology*.

At the outset whilst thanking the author for his book to the Engineering students, one would feel that its usefulness can be enhanced only when the author consults a man on the subject and brings it out on a more rational basis. I hope the author will kindly bear these suggestions while he brings out his next edition.

B. V. IYENGAR.

Records of the Department of Mineralogy, Ceylon—Professional Paper 2, 1944. Colombo, 1945.

This number of the Records contains two papers, one on "Ilmenite, Monazite and Zircon", by D. N. Wadia and the members of the Department of Mineralogy, and the other on "Gems and Semi-Precious Stones of Ceylon", by D. N. Wadia and L. J. D. Fernando.

Hitherto, the only source of information on the commonly occurring Ceylon minerals—

ilmenite, monazite and zircon—has been Sessional Paper VI, published in 1926 by the then Government Mineralogist, Mr. J. S. Coates. During these twenty years much new information regarding these minerals has been obtained, and so this authoritative publication on "Ilmenite, Monazite and Zircon", by the officers of the Department of Mineralogy is to be welcomed. In this paper, the original report by Coates has been thoroughly revised and brought up-to-date. Portions have been entirely re-written and several new analyses have been added.

Ceylon is famous for the abundance and variety of precious and semi-precious stones, for perhaps nowhere in the world are so many minerals of the gem variety concentrated in such a comparatively small area. The second paper on "Gems and Semi-Precious Stones of Ceylon" brings together much valuable information on this interesting subject. The geology of a typical gem field is first described, and then the origin of the gems and their association with the rock systems of Ceylon is discussed. An account is then given of the three methods commonly employed for winning raw gems from the ground. Next follows a systematic description of the gem species and varieties found in Ceylon. The paper concludes with a brief account of the methods used in cutting and polishing gem stones. The paper is illustrated by four plates, the first being a sketch map of gemming areas of Ratnapura District and adjoining regions. The other plates contain diagrammatic sections of gem-fields and gem-pits.

C. S. PICHAMUTHU.

Bulletin 34 : *The Establishment and Early Management of Sown Pastures.* Pp. 210. Illustrations 93.

Bulletin 35 : *The Forage Resources of Latin America—El Salvador.* Pp. 24. Illustrations 5.

These Bulletins are published by the Imperial Bureau of Pastures and Forage Crops, Aberyswyth, Great Britain, prepared in collaboration with the Bureau of Plant Industry, Soils and Agricultural Engineering, and the Forest Service, United States Department of Agriculture, Washington.

The Bulletins are of great value as contributing accurate and important information on the technique of grassland farming in temperate climates, embodying results of scientific experiments and trials, on farm-scale. The countries dealt with are: Great Britain, Canada, Australia, New Zealand, United States of America, with separate chapters for North Eastern States, South Eastern States, North Central Region, Central and Southern Region, Central and Southern Great Plains, North Great Plains, Intermountain Region and South Pacific Coast and Pacific North West Region. Bulletin 35 deals with forage resources of El Salvador in Latin America.

A comprehensive treatment of the subject under severe conditions imposed by varying climatic, geographical and economic aspects over such vast countries has been very successfully attempted, which redounds to the credit of the expert authors and for the method of collaboration at a high level. Practical details on field work, with meticulous care, are given on methods for pre-cultivation, preparation of seed-beds, sowings (even to depth to which each variety of grass seed is to be sown), after cultivation, manures and fertilisers and their placement, rotation and management. Such wealth of detail one often fails to see even in treatises on Indian economic crops.

The Bulletins are profusely illustrated: Bulletin 34 containing as many as ninety-three, which add to the clear understanding of the subject and help appreciation of the types of country each author deals with.

It would only be accentuating the positive if one is tempted to draw the attention of the departments of Agriculture in general and the departments of Animal Husbandry in particular, in India, to the failure, so far, to present any comprehensive literature, much less practical demonstrations on farm-scale, on this most important subject of grass farming. The ever increasing demand for better and more milk not to speak of necessity for livestock improvement in general, is considered to have been met by growing a few well-known grasses like Guinea grass, Rhodes grass, etc., on an agricultural scale to feed the dairy animals in a few well-run dairies. India comprises a wide range of geographic, climatic and economic conditions and so do the varieties of grasses and their growing resources. If national India is to move into direct action to improve its cattle wealth and its food problem the only way, as shown by Great Britain, Canada, America, etc., is to set about practising the technique of grassland farming on the results of broad-based scientific investigations and not to trust, as now, to turning the cattle on to village *Gomals* for grazing, with disastrous results.

K. M. G. RAO.

SCIENCE NOTES AND NEWS

History of Survey of India from Earliest Times to Present Day.—A history of the Survey of India in a series of volumes, entitled "Historical Records of the Survey of India", is being published by the Surveyor-General of India. This series, the first volume of which covering the eighteenth century has now been issued, is being collected and compiled by Col. R. H. Phillimore, formerly of the Survey of India. It is designed to give a full detailed account of the work of the surveyors and geographers of India, and has been prepared from official records of the department, of the Central and Provincial Governments, of the India Office and also from records of the British Museum.

These Records reconstruct the history of surveys in India from the earliest days of their

inception and simple beginnings in the 18th century and aim at bringing it up-to-date.

The first volume of these Historical Records describes the work of the 18th century, "a period of romance and adventure".

The second volume will deal with the period (1800-1815) of the historical development of Indian surveys, when regular organisation and system was brought to the topographical surveys of the Madras Presidency by Colin Mackenzie and the foundation of the trigonometrical survey of India was laid by William Lambton.

The third volume will cover the period (1815-1830), when all the surveys were coordinated under one Surveyor-General of India and Lambton's trigonometrical survey of the South Peninsula was extended as framework for the geography of the Continent at that period. A Revenue Survey Department was established to provide professional control for such surveys, and the great Atlas of India was started to cover the whole of India with a continuous map on a uniform scale.

Indian Aluminium Industry.—The Aluminium industry, a war-born industry, has made spectacular developments during the past three years, and India has now a prominent place among the world producers of aluminium. It is the only non-ferrous metal of which, so far as is known, India possesses large deposits. Rapid developments are taking place in the manufacture and utilisation of this metal.

The year 1943 saw aluminium produced for the first time in India at the Alupuram (Travancore State) Reduction Works of the Indian Aluminium Company. Since then, spectacular developments have taken place. The whole of the war-time requirements were supplied by the Indian Aluminium Company. The rolling mills in Belur, Calcutta, and the manufacturing plants produced sheet metal, and components for aircraft parts, radio and field telephone equipment, range finders, field hospital equipment, etc. From a technical point of view, production operations in the Travancore factory compare favourably with the large production units in Canada and the United States of America. Carbon electrodes required for aluminium reduction are produced in the Works. Arrangements are complete for the production of strong alloys of the duralumin type. The Travancore factory will produce, when its power requirements are fully satisfied, 5,000 tons of aluminium per annum. The construction of the Aluminium Works for the treatment of Indian bauxite at Muri (Bihar) is nearing completion. This factory will have an ultimate capacity of 40,000 tons per annum. The Aluminium Corporation of India, Asansol, has now started working and produces at present about 1,000 tons of aluminium a year. This production will be stepped up shortly.

"Curare to Aid Anæsthetics.—To see whether curare, a deadly poison with which South American Indians tip their darts and arrows, could be used to make anæsthetics safer in operations, Dr. Prescot, Director of Clinical Research at Wellcome Research Institution, allowed himself to be poisoned by it.

His chief collaborators were Dr. Geoffrey Organe and Dr. Stanely Rowbotham, two of the most skilled anæsthetists in England. They described Dr. Prescott's experiences as terrifying. An injection of curare paralysed him; he was unable to speak, swallow, cough or move. When paralysis stopped his breathing, he was revived by artificial respiration. In six hours he had recovered and there have been no after-effects.

Dr. Prescott said: "I knew unconsciousness was coming over me, but I was unable to give any sign. I tried and failed. We have learned a great deal. Properly administered, Curare and its derivatives will prove a great aid in anæsthetics and eliminate many post-operative complications."

Scientist's Plan Against Tse-tse Fly.—Mating calls of tse-tse flies are being recorded on gramophone discs by Mr. F. L. Vanderplank, a biologist engaged in research at the Bristol University on behalf of the Tanganyika authorities devoted to neutralising the scourge of sleeping sickness. Mr. Vanderplank has discovered that the flies send out mating calls by vibration of the wings. By producing an artificial mating call he believes that flies of different species could be induced to mate. The hybrid offspring would be sterile. False calls on a large scale would bring out so much cross-breeding and consequent sterility that the tse-tse fly would become extinct.

Flying Display.—Visitors from all parts of the world will be invited to Great Britain in September to see the Flying Display and Exhibition organized by the Society of British Aircraft Constructors at the Handley Page Aerodrome, Radlett, Hertfordshire. Six such displays were organized by the S.B.A.C. each year from 1932 to 1937.

Two hundred British companies—manufacturers of aircraft, aero-engines, propellers, instruments, components, materials—will show their products. The Society's guests will be shown modern types of commercial and combat aircraft, and the latest power plants, both piston and gas-turbine units. The entire display will cover an area of more than two acres.

For two days—September 12th and 13th—this display will be open to the invited visitors, who will be able to get at first hand a complete review of British aircraft and aviation products. On the second day, there will be a flying display by record-breaking jet propulsion fighters, bombers, and the several new types of civil transports which have come from the British factories since the end of the war.

ERRATUM

In *Current Science*, June 1946, p. 162, *in place of* "Table II shows the effect of changing the inlet acid concentration of nitrobenzene on $K_N a$ and $(H.T.U.)_{ON}$." *read* "Table II shows the effect of inlet acid concentration of acid solution and the influence of extraction height on $K_N a$ and $(H.T.U.)_{ON}$."

943-46 Printed at The Bangalore Press, Bangalore City, by G. Srinivasa Rao, Superintendent and Published by Dr. M. A. Govinda Rau, M.A., Ph.D., for The Current Science Association, Bangalore